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AMERICAN
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CONTENTS

ARTICLES

Political Science and the Crisis of Authoritarianism
Lucian W. Pye ... 3

The International Trade Commission and the Politics of Protectionism
Wendy L. Hansen ... 21

Opportunity, Willingness, and the Diffusion of War
Randolph M. Siverson & Harvey Starr ... 47

Legitimacy, Religion, and Nationalism in the Middle East
G. Hossein Razi ... 69

Communication, Ideology, and Democratic Theory
James F. Bohman ... 93

Mandates and Policy Outputs: U.S. Party Platforms and Federal Expenditures
Ian Budge & Richard I. Hofferbert ... 111

Cooperation by Design: Leadership, Structure, and Collective Dilemmas
William T. Bianco & Robert H. Bates ... 133

Are Congressional Committees Composed of Preference Outliers?
Keith Krehbiel ... 149

Sequential Voting with Endogenous Voter Forecasts
Dennis Epple & Joseph B. Kadane ... 165

Missing the Archimedean Point: Liberalism's Institutional Presuppositions
Robert Grafstein ... 177

Evolutionary Biology and Political Theory
Roger D. Masters ... 195

RESEARCH NOTES

Victims of Circumstances: Women in Pursuit of Political Office
Timothy Bledsoe & Mary Herring ... 213

The Electoral Relevance of Local Party Organizations
John P. Frendreis, James L. Gibson & Laura L. Vertz ... 225

The Strategy of Candidate Ambiguity
Amihai Glazer ... 237

Asymmetric Information and the Coherence of Legislation: A Correction
David Austen-Smith & William H. Riker 243

CONTROVERSY

Political Culture and Political Change
Herbert H. Werlin & Harry Eckstein 249

BOOK REVIEWS

Science, Rights, and Control of the Body 269
Political Theory 275
American Politics 290
Comparative Politics 330
International Relations 354

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ARTICLES

POLITICAL SCIENCE AND THE CRISIS OF AUTHORITARIANISM

LUCIAN W. PYE
Massachusetts Institute of Technology

Political science is a discipline in constant danger of fragmentation because of the centrifugal pulls of our subfields and the contradictions in our scientific and humanistic traditions. We are, however, periodically brought together by the need to respond to major developments that are reshaping the political universe. We are today confronted with a unifying challenge in the crisis of authoritarianism that is undermining the legitimacy of all types of authoritarian systems throughout the world, including the Marxist-Leninist regimes. The crisis will not necessarily produce democracies, but rather a variety of part-free, part-authoritarian systems which do not conform to our classical typologies. Although the crisis of authoritarianism stems from profound social, economic, and cultural trends, the outcome in each case will be decided by political responses. Political science, therefore, has the responsibility to lead intellectually other social sciences in analyzing the fundamental change in political life that involves the clash between individual political cultures and the world culture of modernization.

Convention holds that one task of the presidential address of a learned society should be to comment on the state of the discipline, to point where progress may lie, engage in some good-natured scolding for any collective failures, and if self-congratulations be in order, to walk with dignity the narrow line between boastfulness and modesty. My job has been greatly simplified by our ingenious practice of scheduling the awards ceremony right before, so that we need not rely on the president's musings for an evaluation of the discipline's standing; but are given empirical evidence as to exactly where it stands, where solid achievement is taking place. All that is left for the president to do is say, "Behold and marvel at the vitality and creativity of the profession of political science!" There seems to be no limit either to the number of worthy recipients or to the number of famous names to designate the honors by, only a limit on funds for endowing them. Moreover, we also have a guaranteed audience for the presidential address and have scheduled the presidential reception (with free drinks) right afterwards, a not-so-subtle hint to be brief.

While applauding the accomplishments of those who have been appropriately honored, we cannot help noting that the extraordinary diversity of the numerous subfields that make up our discipline obscures where the core of our discipline may lie and even raises the question whether enough is still shared to provide a common identity for those who call themselves political scientists.[1] In addressing the International Political Science Association, Gabriel Almond (1988) used the vivid metaphor of "separate tables" to warn us that our cliquishness in methodology and ideology has become a major obstacle to the advancement of knowledge.

In an earlier and more simple era, our collective baptism in political theory and thought gave us a shared experience. I suspect that in spite of current complaints about philosophical illiteracy we are probably still bonded together more than we suspect by that heritage. We then passed through the heady times when the dreams of the behavioral revolution held out the vision that by collectively pursuing the scientific method and adhering to the canons of quantification we would be able to generate cumulative knowledge just as chemists and physicists had long done. But then came the rude shock that although we would have to follow the same laborious procedures of collecting and analyzing data as the physical scientists do, our findings have appallingly short half-lives. The ambition to discover universal and enduring laws like Boyle's law has been frustrated by the realization that human behavior is too sensitive to the fluctuations of culture and the circumstances of history to yield permanently enduring findings. Instead of a need for replication for purposes of verification, our problem has been a need to replicate, because with passage of time social reality inevitably changes.

Thus, today, neither content nor method provides an "essential core" for the discipline as a whole. There are, of course, many reasons why the various subfields have gone off in their separate directions; but certainly part of the current confusion arises from the determination of political scientists to employ both scientific and humanistic approaches without a consensus as to where the emphasis should lie, in spite of the word *science* in our name. Some of the subfields—some of *us*—are convinced that true knowledge can be found only in the rigorous search for invariances, for the regularities we expect to find in nature. In this spirit we like to think of man and society as having the lawlike qualities we associate with the physical universe. For other subfields—and others of us—knowledge is the search for meaning, for understanding and interpretation, that is, for what is human in the blending of mind and spirit. This is the understanding of phenomena that leads to humanistic learning and wisdom.[2]

As a part of our confusion over scientific and humanistic knowledge we tend regularly to vacillate between favoring the one or the other. Indeed, the subfields are usually at different phases in what seem to be steady pendulum swings between the universal and the particular, theory and description, parsimony and "thick description."

At one time or another most of the fields have experienced the exhilaration of being in possession of elegant formulations promising universalistic knowledge. But then came the process of amendment, elaboration, and qualification to ever-greater complexity. In the end, the only alternative seems to be to seek understanding through in-depth analysis of particular cases; for, as the novelists say, "God is in the details."

In time, however, "thick description" becomes tedious and the mind craves the power and elegance of generalizable theories. Clear logical theories then have their attractions for a while until they lose their charm for being too simplistic and mechanical. It is said that Niels Bohr never trusted a purely formal or mathematical argument, "No, no," he would say. "You are not thinking; you are just being logical" (Margolis 1987, 1).[3]

The Crisis of Authoritarianism and a New "Great Transformation"

Fortunately, however, our tendency toward fragmentation is countered by another characteristic of the discipline, namely, our sensitivity to problems and developments in the real world of public affairs. Every now and then we are pulled back from going our separate ways and our collective mind becomes concentrated by major current events, for example, by the emergence of the new states after World War II, the beginning of the Cold War, the turmoil of the inner cities, the problems of the governability of the advanced industrial societies, and the like. I believe that political science is today faced with just such a new challenge that is profound enough to unite the discipline because it raises problems that call for contributions from nearly all our subfields. This new challenge is that of analyzing and explaining the crisis of authoritarianism that during the last decade has been sweeping the world, bringing into question both the legitimacy and the competence of all manner of authoritarian systems. The crisis has produced a variety of responses. In some cases there have been decisive transitions from autocratic rule toward democracy. In other cases the legacy of failure of the autocrats leaves the new regimes with almost insurmountable problems, as with the debt burdens and the hyperinflation of several Latin American countries. In still other cases the authoritarian regimes themselves seek to initiate reforms and liberalize practices. Finally—as tragically in China—determined hardliners seek to overcome the crisis by increased repression and the adoption of a siege mentality.

Indeed, the crisis of authoritarianism reached dramatic heights in the Beijing Spring of 1989 when China shook the world, first with the heartwarming spectacle of the students demonstrating for democracy in Tiananmen Square and then with the horrors of an autocratic regime flaunting its power by massacring its citizens and hypocritically pretending that the nonviolent demonstrators were "thugs" and "hooligans" and that the casualties were mainly the heavily armed soldiers. Concurrently in the Soviet Union the unthinkable was happening. In a one-party state the Party's candidates, even when running unopposed, were defeated in elections to the new Congress of the People's Deputies; and the winners dared attack the KGB as an "evil" service that was a "threat to democracy." The deputies to the new Supreme Soviet stopped being rubber stamps, boldly rejected the candidacy of a deputy premier, and refused to confirm three ministers nominated by the Party. And as though to prove that wonders never end, in the last week of August 1989 a non-communist government replaced the communist dictatorship in socialist Poland; and we now await the historically profound consequences.

These are astonishing events, but behind them are historic forces at work today causing acute crises for all types of authoritarian regimes. The stigma of failure marks every form of tyranny, whether of the one-man or the one-party variety. As a result, profound changes have occurred in once-static authoritarian regimes, from those of generals and old-fashioned autocrats to the most advanced of the Marxist-Leninist systems. Dictatorial rule has failed to deliver on its promises of purposeful efficiency in all the regions of the world, from Southern Europe to Latin America, from Eastern Europe to China, from the Philippines to South Korea and Taiwan, and most significant of all, perhaps, in the Soviet Union, the system that was once the archetype of totalitarianism.[4]

We are witnessing a "great transformation" that is probably more profound than the one Karl Polanyi (1944) described with that phrase. Polanyi wrote of the "great transformation" that came with the collapse of nineteenth-century European civilization and the emergence of a world capitalistic system based on the idea of a self-regulating world market that, in his judgment, was destined to erode the traditional bonds of European society. Are we not possibly today at another great turning point in history when, contrary to Polanyi's predictions, transnational forces involving a dynamic world economy and revolutions in technology and information are compelling authoritarian governments to open up their economies and relax their political controls?

At present what seems to be at work is a complex interaction between a much more dynamic, but not yet clearly institutionalized, international system and the individual nation-states that we have traditionally viewed as relatively autonomous. Sovereignty still prevails in some domains, but a host of international or transnational forces are simultaneously at work, affecting how people act in what were once considered to be domestic affairs, and above all, impinging on the priorities of government. All governments are put under pressure by the increasingly significant flows of international trade, finance, and communications; by the effects of contemporary science and technology; and by all the other elements that make up what we imprecisely call modernization. But the authoritarian regimes are the most vulnerable and are therefore being seriously undermined.

It is true that in some respects we live in a more nation-bound, less internationalized world than did the Europeans of the nineteenth century. It is, for example, astonishing that it was not considered odd for the tsarist government to raise the funds for fighting the Crimean War by floating bonds on the London market, the same market that the British government was using to cover its own expenses in the same war. Possibly if the Vietnam War had gone on longer and the polarization had become greater, critics of the war might have tried to sell bonds for Hanoi on the New York market. But in the case of the Crimean War it was done without rancor or passion, simply because that was the proper way to do such things.

Intellectually, however, we have not advanced far in integrating the analysis of domestic developments with the transnational processes that are shaping our new world. In reporting on an attempt of the Social Science Research Council to confront this problem of foreign area studies and comparative and transnational research, Frederic Wakeman observes that "periods of the late 19th century or the 1950s and 1960s, in which we have had clear conceptualizations of what the social sciences ought to be doing, have been periods in which the world itself appeared to be much clearer, at least in terms of architechtonic world structures. As these structures lose their clarity, so do the categories of social science lose their limpidity and rigor. We are in a period today, both conceptually and structurally, that is characterized by imprecision and amorphousness" (1988, 87).

Part of our difficulties stems from forgetting Tocqueville's prescient insights about political modernization. Viewing the U.S. political scene, he declared that the inexorable forces of history were on the side of democracy and that rule from above would inevitably give way to the sovereignty of the people. The nineteenth century gave promise to Tocqueville's prophecy, as constitutional republics seemed to be the wave of the future; and political science responded with buoyant optimism as scholars prophesied Progress and Enlightenment. But with the twentieth century and its violent sequences of wars, revolutions, and repressions, Tocqueville's prophecy was brought into doubt as the future increasingly seemed to lie in the choice between authoritarianism and totalitarianism. Both fascism and Marxism-Leninism hypocritically mocked Tocqueville by pretending to be acting in the name of the "people" and the very label *people's republic* became the code word for repression. The conventional wisdom of the 1950s and 1960s held that communist governments and authoritarian rulers had certain advantages over democracies in speeding economic development. Indeed, as late as the early 1970s it seemed likely that any crisis of an authoritarian regime would probably open the way to a harsher and more totalitarian system. The 1973 coup against the enfeebled strong man Haile Selassie brought the radicalized Marxist Derg; Samosa's dictatorship was replaced by the Marxist Sandinista revolution just as Batista was followed by Castro; and the coups in Afghanistan brought Soviet-backed rule.

However, by the end of the decade history seemed to be teaching a new lesson. The presumed advantages of totalitarian practices for economic development have apparently evaporated. Now the spreading belief is that progress depends upon liberalization and deregulation. A benchmark for charting this historical trend was 1975, which saw the Portuguese revolution, the death of Franco in Spain, and the fall of the colonels in Greece—developments that brought democracy to all of Western Europe. As scholars analyzed these developments, along with the military's return to the barracks in much of Latin America, we were early on presented with theories about the processes of transition from authoritarian rule, challenging the earlier assertions that the international capitalist system was a principal cause of authoritarianism in the Third World.

The Vindication of Modernization Theory

There are, of course, multiple forces at work shaping the current "great transformation." The key factors were all identified as critical variables by the early modernization and political development theorists. Where we went wrong in the 1950s and 1960s was in grossly underestimating the magnitude these factors of change would acquire in the decades ahead and the extent to which they would become part of closely knit international systems. Modernization theory predicted that such developments as economic growth, the spread of science and technology, the acceleration and spread of communications, and the establishment of educational systems would all contribute to political change. We cannot here document all the ways these factors have brought about the current crisis of authoritarianism, but we can note briefly the extent to which we failed initially to appreciate the orders of magnitude they would reach.

For example, the exponential growth of the world economy in the last decades greatly exceeded the predictions about world trade implicit in both neoclassical and Marxist theories. International trade has grown at a compounded rate of 13% per year over the last 25 years, whereas before it never grew at more than 3%. Even more impressively, international banking transactions have compounded at an annual rate of almost 28% per year; and during that period the Eurocurrency market alone mushroomed from seven billion to 3 trillion dollars.

To the astonishment of the world—especially ideological Marxists—the leaders of the "socialist" countries have swallowed their words of condemnation about the once abominable "capitalist-imperialists" and have sought to outdo each other in seducing their one-time foes into joint ventures and even direct investments. In the exuberance that followed Deng Xiaoping's "opening" to the outside world, the United States alone in one decade has put more capital into China than the total of all the European, U.S., and Japanese investments in the 1930s, when China considered itself a victim of Western imperialism.[5] Of course, it is true that historically the scale of all economies was smaller; but this is not a telling objection because before World War II foreign trade and investments affected barely 2% of China's GNP, whereas just before the Tiananmen Massacre over 27% of China's GNP was so traceable.

The revolution in science and technology has pushed these dramatic changes in the world economy. It has estimated that the generation since World War II has "added seven times as much to the world's producing power as was added during all the previous millennia of *homo sapiens* existence" (Norman Macrae, "The Next Age of Man," *Economist*, 24 December 1988, 20). With scientific knowledge more than doubling in each of the last four decades the pace of technological change has accelerated at a comparable rate. Although it is true that technology has no ideology, this growth of knowledge has had a profound impact on the character of education, which in turn has brought new generations of technocratically skilled specialists into all governments, including the authoritarian ones. These are people who see governing as technical problem solving. Thirty years ago the governing elite in Taiwan had no Ph.D.s and more hadn't completed high school than had graduated college, whereas now there are more Ph.D.s than people with only a high school education. Forty-five percent of the current Thirteenth Central Committee of the Kuomintang have postgraduate degrees, and only 2% have merely a high school education or below (Li and White 1988.) The drama of the spring of 1989 in Beijing was in part fueled by the frustrations of a generation of better-educated students confronting an elite dominated by aged leaders with little formal education beyond elementary school.

The revolution in communications has gone far beyond the impact of radio, which Daniel Lerner (1958) saw as a key instrument for modernization. The invention of microchips and satellites has meant not only that authoritarian rulers find it harder and harder to isolate their countries from the intellectual and cultural trends sweeping the world but that their own actions are instantly played back to them and to their people. Thus, Ferdinand Marcos made his politically fatal mistake of promising an election while on U.S. television. The Chinese leaders were initially inhibited from taking instant action against the demonstrating students in Tiananmen Square because of the worldwide television coverage of Gorbachev's visit to Beijing. The Chinese students demonstrating for democracy were receiving each day faxed copies of the editorials and news reports of the *New York Times*, *Washington Post*, *Boston Globe*, *Los Angeles Times*, and the *San Francisco Chronicle* only minutes after they reached the streets of U.S. cities. Indeed, the miracle of satellite television made the dream of Tiananmen Square a superspectacle that enthralled and then dismayed a worldwide public for nearly two months and made the Deng-Li clique's attempt at the Big Lie so obscene, so impossible to get away with.

In the last decades the exponential growth in all of the variables that energize the modernization process has stimulated a lot of imaginative speculation. Some have sought to encapsulate what has been taking place by theorizing about a knowledge, or information, revolution. The study of advanced industrial societies has been pushed to the point of theories about postindustrial societies. More specifically, there has been speculation about the "end of communism." Clearly, the long historical trend that favored the strengthening of centralized state power has seemingly come to an end, and the trend now favors the pluralism of decentralized authority. The dramatic increase in the availability and the importance of information, made possible by the ability of computers to store huge quantities of information and retrieve it almost instantaneously, has changed first the nature of economic activities and secondly governmental processes. The revolution initially took place, of course, in open societies; and it quickly accelerated the gap in the pace of modernization between open and closed societies. As a consequence, the costs of repression for authoritarian rulers rose sharply, causing most of them to seek at least halfway measures of liberalization. Indeed, the information revolution calls for decentralization and a diffusion of power throughout the society incompatibly with centralized authoritarian rule, thereby sharpening the crisis of authority.

Let us repeat that what is happening does not mean that we have seen the end of authoritarianism or of Marxism-Leninism. Of course, there will be new dictatorships, for the instincts that produce tyrannies will not easily disappear from the human spirit. And the peculiar appeal of Marxism-Leninism, which was able to overcome the Moscow show trials, the Gulag, and the worst famine in human history caused by Mao's Great Leap Forward, will no doubt continue to attract some faithful followers. The repression in China after Tiananmen provides sad warnings that during periods of surface liberalization the secret police and the thought control apparatus can still remain in place, quietly going about their work. There is no guarantee that democratic yearnings will produce true democracies in the newly industrializing countries that have taken the first steps away from authoritarian rule. Nor is it certain that market forces will win out over the bureaucracies that are well entrenched in centrally planned, command economies. On the contrary, there are good grounds for questioning whether the Chinese economic "reforms" will work and for skepticism about whether *perestroika* will "succeed."

But what is different is that the forces of modernization have made it harder for political willpower to mobilize and dominate a society. Indeed, at hand is the end of the time when populations can be easily convinced that sheer political willpower—be it the will of the leader, the party, or the state—is enough to transform societies. Instead, there is a growing awareness that authoritarian rule, whether Leninist or not, can be a liability in achieving progress. The idea that centralized authority enhances the state's ability to shape society has been dealt a devastating blow by the record of performance of the states that have tried to carry the idea to the extreme.

Aside from the potency of coercion, authoritarianism always needs to exploit the legitimacy inherent in proper hierarchical human relationships, that is, the instinctive understanding that inferiors can expect benefits from yielding to the will of benevolent superiors. The modernization process inevitably changes the character of this relationship between subjects and rulers so that the benefits increasingly seem smaller and the costs of forgoing freedom greater. The emergence of a middle class and the growth of a technically educated population create new centers of power, leading to drastically altered attitudes about the nature of authority.

It is unlikely, for example, that South Korea and Taiwan could have put off political reform indefinitely, given their rates of development. South Korea, a war-torn, shattered society only thirty years ago, now has a population of which 61% say that they belong to the middle class. Newspaper readership in both Korea and Taiwan is now on a par with that in the United States, and the sophistication of that readership is such that in both countries the press has had to become more free. In the Soviet Union, behind the strange mixture of failures and pretensions of the post-Khrushchev years of stagnation, profound social changes have in fact taken place. The failures are of course manifest: a dilapidated infrastructure, run-down housing, shortages of consumer goods, pathetic levels of agricultural production, endless standing in line to buy anything worth buying, declining life expectancies, a budget deficit that in proportion to GNP is three times that of the United States—in short, a Third World country with a nuclear arsenal and men floating in space.

Yet during this era of failures, Soviet society made the great transition from being a predominantly rural people to an overwhelmingly urbanized country. At the end of World War II, half of the Russian people were peasants. Now only 12.5% are peasants; and judging from Gorbachev's continuing problems with agriculture, this residual class of peasants would seem to be disproportionately made up of dullards, people with little ambition and few skills. The levels of education in the Soviet Union have dramatically increased. In 1959, 91.3% of the workers and 98.2% of the *kolkhoz* peasants had only an elementary school education. By 1984 only 18.5% of all manual workers had not gone beyond elementary school (Lewin 1988, 47). Even more significant has been the growth of what the Soviets call their "intelligentsia" or "specialists." In 1941 these technically trained people numbered only 2.4 million. By 1960 they had grown to 8 million, and now they total 31.5 million, constituting some 40% of the urban population. Seweryn Bialer has collected figures to show the Soviet Union to be "a society with a professional class that is, numerically, the largest in the world" (Bialer 1988, 407). Yet there were great deficiencies in the training of technocrats, in that the Soviets failed to produce the full range of modern economists, sociologists, and political scientists who might now provide professional guidance on how to bring life to their stalled economy and their faltering political system.[6]

These and other figures that I could cite point clearly to a basic problem Gorbachev faces with his *perestroika* and *glasnost* efforts. A significant middle class has emerged, the members of which demand greater intellectual and cultural freedoms and are contemptuous of the poorly educated bureaucrats and party apparatchiks, that is, most of the *nomenklatura*. But there are also vast numbers of people who don't want to shake things up for fear of losing what they have just gained. These cautious people, combined with the undereducated *praktiki* of an older generation, constitute major obstacles to truly radical economic reforms. At the same time the liberalizing of politics has made people ever more sensitive to their feelings of group identity, hence the explosion of the nationalities problem.

The situation in China is of course quite different. The disaster of Mao's rule and his hostility toward formal education left that country with an appalling shortage of technically trained people. Indeed, the Cultural Revolution resulted in the loss of an entire generation. The universal acknowledgement that things had gone wrong for much too long produced an erosion of legitimacy that made Deng Xiaoping's task of initiating reforms politically easier than Gorbachev's. The Maoist system had undeniably been a failure. After decades of hardship and unmatched sacrifices that had produced only propagandistic glory, China was left with a per capita income that put it among the twenty lowest countries in the world—about equal to Somalia and Tanzania. Therefore, desperate efforts would have to be made to change the economy. The Chinese population was still some 80% rural, and the peasants had not lost the art of skillful farming; so China's initial agricultural reforms came with ease. However, with the shortages of technically trained people and the gross neglect of the infrastructure, China's urban and industrial reforms turned out to be more difficult. By 1988 the economy began to falter, price reform stalled, the leadership seemed adrift, inflation took over, corruption abounded, and when professors realized that they were earning less than waitresses and cab drivers, the students knew that the country was in trouble. Worse still, the leadership seemed to be of two minds about the value of modern skills. While sending tens of thousands of students abroad, it also set wage rates in which the ratio between manual and technically skilled labor was less favorable for the latter than in the Mao years. And since Tiananmen it has begun punishing students in ways that can only hurt the country's future development.

The turmoil of the Cultural Revolution and the subsequent crisis of legitimacy, however, also reinforced within the leadership the deep fears of anarchy and disorder that have been central features of the traditional Chinese political culture. Consequently Deng Xiaoping and his appointed heirs began moving in two contradictory directions. While willing to be bold and experimental in carrying out economic reforms, the "ten years of disaster" made them extremely hesitant about liberalizing the political system. By the mid-1980s Deng Xiaoping's failure to provide a political vision for China produced an increasingly acute crisis of legitimacy that was for a time papered over by a spirit of crass materialism and shameless corruption. As the economy deteriorated and the leadership seemed confused and divided, China became a tinderbox that was easily exploded by the match lit by the student demonstrations of April and May 1989. Deng's false idea that political stagnation could provide a healthy environment for economic growth produced disillusionment and frustration for all concerned—students, citizens, and the rulers themselves.[7] Yet although societal changes in China have not gone far enough to prevent serious backsliding, they have advanced far enough to compel the regime to proclaim its continuing commitment to the "reforms" and its "open door" economic policies.

The Imperatives of Technology Clash with the Forces of Politics

We cannot here sort out all the differences and similarities in the various crises of authoritarianism, nor can we determine the prospects of success for the efforts at reform, especially among the Marxist-Leninist systems: "It took kings a long time to travel from divine right to constitutional monarchy. How long will it take the twentieth century's claimant to kingly power, the communist movement?" ("Just a Party among Others? When Is a Communist Party Not Communist?" *Economist,* 18–24 March 1989, 46). The process is difficult because in all the crises in authoritarianism there is a fundamental clash between the culture of modernization (what I have called the world culture) and the various national political cultures. This is always a psychologically disturbing clash because it brings into confrontation universal standards and parochial values. The former are essential for economic performance and effectiveness, and the latter are critical for creating national loyalties and distinctive national political styles. Arrayed on one side are the technocratic considerations imposed by the combined imperatives of economics, technology, and the principles of efficiency associated with rational public policy. Confronting these impersonal forces there stands the basic need of every polity for a sense of self-identity, for the dignity that comes from a fundamental belief in the collective uniqueness of the national entity and its spirit of community.

This clash has produced a particularly acute crisis of faith in the Marxist-Leninist systems precisely because their ideology long informed the faithful that there could be no such contradiction for them. They were promised that through their identification with "scientific Marxism" they had been given a political identity that was universalistic and at the forefront of human progress. The parochialism of

Marxism has been its claim to a universal theory of history.[8] National cultures have, however, proved to be too strong, as seen in the intractable ethnic problems of the Soviet Union, the current Chinese goal of "building socialism with *Chinese characteristics,*" and the triumph of Polish nationalism.

The advances of modernization and the spread of the world culture inexorably place constraints on the domain of politics. Before events made it unmistakably clear that progress depends heavily on the knowledge of technocrats, it was possible for leaders in the Third World to proceed as though the thrill of politics would be enough to conquer all obstacles to national desires. Mao Zedong, with his faith in the efficacy of political willpower, accepted Rosa Luxemberg's view that "revolution is magnificent, all else is rubbish." Kwame Nkrumah had his slogan, "Gain ye first the kingdom of politics and all else will follow." Both were prophets of modernization; but both were false prophets, for the truth is that whereas modernization can expand the potential for human growth, it also places limits on the free play of ideology. In country after country ideological politics has had to yield to pragmatic decision making.

We must not, however, exaggerate the constraining powers of economics and technology. The political world is the world of power, and at times all else must yield to the limitations imposed by the imperatives of political feasibility. The rationality of economics and engineering must give way to the realities of what is politically possible and what is politically unacceptable. In every polity there are generally a host of problems that would seem, from a technical point of view, to be readily solvable but that cannot be effectively dealt with because of political considerations. Moreover, the very processes of institutionalization associated with political development give increasing precedence to political considerations.

All of this means that the process of modernization must take place in the context of constant battling between two nearly sovereign forces—the impersonal and universalistic requirements of the world culture and the particularistic passions of politics and of group identity. The outcome of this clash (and hence the outcome of any particular crisis of authoritarianism) will depend on the character of the political culture, and the extent to which it either moderates and accommodates the conflict or exacerbates it. All cultures are to some degree protective of the integrity of their indigenous uniqueness. Some, however, are hypersensitive about perceived threats of foreign ideas and practices while others are more adaptable. The differences are critical in determining the character of the nationalism that inevitably accompanies political development. Indeed, the basic contradiction of political development is that it requires not only the opening of doors to outside influences but also the protection of nativist sentiments of national identity. It is the mark of most late-developing countries that their cultures have great difficulty in managing this contradiction.

U.S. citizens generally find it hard to appreciate the psychological reactions that this clash between the world culture and the national culture can produce in other societies because one feature of "American exceptionalism" is that historically, we were spared this tension because the progressive movement that sought political modernization came as a nativist development and was not seen as a foreign or cosmopolitan influence.

The Search for Theories to Guide and Evaluate Transitions toward Politics

I am arguing that our understanding of the likely outcome of the various crises of authoritarianism calls for a breakthrough in theory building so that we can better understand the problems of the interrelationship of the universal world culture and the particularistic national cultures. This challenge lies at the core of the problem of the relationship between theory and description, between the universal and the particular, a troublesome problem for our discipline. Our theory building needs to be directed to finding a basis for comparative analysis that can integrate the descriptions of particular cultures better with the generalized concepts necessary for analysis. We need to realize a closer fit between the vividness of the concrete and the powers of the human imagination that can lead to explanatory theory.[9]

As a first step in such theory building we need finer shades of typologies of political systems between the classical polar opposites of authoritarian and democratic. In the wake of the crisis of authoritarianism we can expect a wide variety of systems that will become part authoritarian and part free and that will fall far short of any reasonable definitions of democracy. The richer typologies should identify types of systems that represent varying states of equilibrium along the continuum from authoritarian to democratic. In some of the cases democratic pretensions will mask latent authoritarian practices, in others there may be genuine tutelage toward democracy. In still other cases efforts at repression will seek to hide a drift toward collapse and conditions of near anarchy. The desperate efforts of both Gorbachev and Deng to cope with their respective crises of authoritarianism makes urgent the need to identify both when reforms have indeed become irreversible and what the sticking points in any downward spirals toward collapse may be.

Although the two polar authoritarian and democratic extremes probably have a higher potential for stable equilibrium than any of the stages in between, we still, in order to evaluate the prospects of post-authoritarian systems, need to learn more about conditions that can produce relatively lasting equilibrium in a society that is part free and part authoritarian. Through the works of Eric Fromm and Hannah Arendt, among others, we know something about the distinctive human or cultural basis of totalitarianism; and through the works of Almond and Verba, among others, we also know something about the civic culture basic to stable democracy. Now we need to know more about the cultural bases for maintaining types of systems that lie between these two extremes.

Beyond this general challenge for comparative analysis there is a special problem in the comparative study of post-authoritarian systems, which is the need to understand the psychodynamics of the widespread fear associated with political repression and the likely reactions to the lifting of terror. Montesquieu long ago taught us that fear was the key sentiment basic to tyrannies; and while we now, sadly, have abundant experiences with state-induced fear, we are very short of knowledge about the aftereffects of reigns of terror. In some cultures the receding of fear of state power seems to ignite hope, which in turn can open the floodgates to enthusiasm and activism. The resulting state of public mind is one that liberalizing authorities may welcome and exploit, as in Taiwan and Korea, or find threatening, as in China with the Tiananmen incident, when hope and enthusiasm gave way to disillusionment and anger and when, in the words of the Chinese poet Li Shizheng, "we hear again the urgent knocking of red terror" (Li 1989).

In other cultures fear may be followed by cynicism, as for example among many Russian intellectuals of today. At the core of cynicism lies passivity; thus, shockingly bitter criticism may abound without any rebellious actions. As Jeffrey Goldfarb points out in *Beyond Glastnost: The Post-Totalitarian Mind* (1989) and Miklos Haraszti brilliantly documents in *The Velvet Prison* (1987), cynicism can be manipulated so that the goals of a discredited totalitarianism can be served by seemingly antitotalitarian actions. Intellectuals in particular may be allowed enough freedom that they rejoice over the change, and outside observers can be astonished as they also failed to reflect on how far conditions still are from real freedom. Deng Xiaoping's rule was such a great improvement over that of Mao Zedong that people forgot until Tiananmen that China was still far from an open society.

We need to be careful that extolling honesty in the United States—the value that Montesquieu said was the key to democratic government—does not blind us to the fact that cynicism when combined with institutionalized hypocrisy has historically served as a remarkably durable basis for political legitimacy. Hence it is quite likely that many cultures, such as some in Latin America and possibly the Philippines and even India and the Soviet Union, will be able to combine high levels of cynicism with unexpected degrees of political stability.

A special problem in all postauthoritarian developments is the relationship between the political and the economic systems. It is in the nature of all authoritarian systems to use political power to determine who is to benefit and who is to lose out economically. Hence, in all serious reforms associated with the crisis of authoritarianism there must be new winners and new losers. Therefore, economic decisions will be governed by political considerations. Given the central importance of politics in such situations, we should not as political scientists assume that the economists can lead the way in forecasting the prospects of reform. It is understandable that economists have not developed theories about the optimum policies to follow in seeking to transform a centrally planned economy into a market-oriented one because the issues involved are essentially political ones.[10]

The dominance of the political over the economic can be seen in what the World Bank has identified as "transfer economies," which are different from either market or command economies. These are economies in which the state transfers substantial resources—extracted from the society—by means of extensive subsidies that favor some elements of society at the expense of others but do not add to productivity or contribute to building up the infrastructure. The result is inadequate investment rates, deteriorating infrastructures, and widespread inflation (as in the USSR and China) if not hyperinflation (as in many of the newly democratic states in Latin America).[11]

Beyond the initial task of establishing appropriate typologies of postauthoritarian regimes, the next step should be the theoretical analysis of the conditions that will bring about transitions from one state of equilibrium to another. This will require reducing the customary gap between our generalized analysis of political change on the one hand and our understanding of the particular, and apparently unique, developments in individual countries on the other. Theory must be sensitive to cultural nuances, while country and regional specialists need to explain in more generalized terms the distinctive features of their specific cases. In terms of Giovanni Sartori's three levels of abstraction, we should stress the high level of universal conceptualization less and seek to generate theories at the medium level of taxonomies and at the low level of configurative conceptualization (Sartori 1970).

If we successfully move in such directions, which would be in line with Lasswell's call for configurative analysis, healthy advances on two fronts should follow. First, we might expect greater sophistication in the art of describing particular cases, so that there will be fewer attempts to overwhelm with details and more effort given to fitting the details to theoretical concepts of causality. Second, our theory building would have stronger empirical foundations and seem less artificial to those with detailed knowledge of the concrete situations in the particular countries.

In searching for the most promising variables for explaining dynamic change we probably could do no better than to start with Robert Dahl's theory of polyarchy, more particularly his seven sets of conditions that are most and least favorable to polyarchy (Dahl 1979). These can be combined with the elements of the dynamic model of the processes of change that Dankwart Rustow proposed in one of the earliest discussions of transitions to democracy (Rustow 1970). Additional factors that would have to be included are of course the basic political cultures and historical traditions. There are, for example, significant differences in the reaction to the authoritarian crisis between countries that had earlier experiences with democratic institutions and those knowing only authoritarian rule.

The Shock of Failure and the Importance of Politics

I cannot attempt here to go beyond these preliminary remarks about a general theory for evaluating the transition from authoritarianism, but I do want to note briefly an exceptionally important consideration that might be easily overlooked. This is the question of the intensity with which both the leaders and the general population have experienced a psychological shock in recognizing the failure of authoritarianism. This shock of failure is critical in weakening the legitimacy of authoritarian elites and in raising the consciousness of the public enough to cause the people to reflect on their country's troubles.[12]

The impact of failure on authoritarian regimes is important for two other reasons. First, the awareness of failure should teach the lesson that leaders are fallible and not omnipotent. Leaders need to know that it is human to err. They also need to learn that out of this first lesson comes a second, namely, that it is thinkable to accept the risks of allowing power to be transferred to others who can then have their turn at being fallible. In the communist world, in varying degrees, the first lesson seems to be sinking in. Faith in the Party's monopoly of truth has faded, but the desire to monopolize power has not. Belief in a Lenin-given right to rule lingers on.

To the degree that leaders and their publics absorb these two lessons of failure it is possible for the society to realize true politics. As Bernard Crick has said, "politics is a great and civilizing human activity" (Crick 1972, 15). By *politics* I mean the activity about which Crick has written so eloquently, an activity that all political scientists should, I believe, instinctively appreciate. It is an activity through which contending interests are conciliated, differences are expressed and reconsidered, the collective welfare ensured, and the survival of the whole community protected (Crick 1972, 22). Politics allows for divisions but it also unites collectivities. In authoritarian systems there is only a limited form of politics, for power struggles among factions in one-party regimes and disagreements among soldiers or bureaucrats are not the same as real politics, which must operate in the context of a civic culture. The movement toward politics calls for competition in adversarial relationships among parties who accept the idea that no one has a monopoly on absolute truth and that there can be no single, correct answer to public policy issues.

After years of authoritarianism it may

be hard for people to accept the uncertainties of a society organized around politics. Moreover, some political cultures are built upon traditional ideals that are disdainful of the spirit of competition and compromise basic to real politics. In some cases it is a problem of exaggerated notions of honor, dignity, and status gradations (as with some Latin cultures); in others it is an elitism based on presumptions of virtue and self-righteousness (as with the former Confucian cultures). In all cases the cultural concepts become obstacles for the development of that ethic of responsibility that Max Weber identified as the key to "politics as a vocation."

Yet with modernization and the inevitable growth in the complexities of societies, the only hope for the long-run stability of any society is learning how to manage the disorderliness of politics. In the Soviet Union we can now see the emergence of politics with the increasing manifestation of actual division of powers between the legislative and the executive, as well as the gradual formation of latent parties among the delegates to the Supreme Soviet.

By making the development of politics the critical test for evaluating transitional systems we can avoid the charge of ethnocentricism that might be raised if we used as the standard the narrow concept of Western democracy. Politics stresses process and cultural orientations, and it is compatible with the values basic to democracy; but it does not rely upon any particular institutions or practices, as is the case with democracy.

Political scientists seeking to learn about the authoritarian crisis need to go beyond the analysis of structures and processes and explore the human meaning of politics. To get at the psychology or personality characteristics necessary for carrying out politics we need to go beyond delineating "political man" as the exceptional power seeker and try to unearth the personal qualities essential for the civilized activity that is true politics. Politics calls for people who relish the challenge of adversarial relations but who can also control their aggression and manifest mutual respect.

In an odd way, the moral of politics for countries experiencing the crisis of authoritarianism is a moral that is also applicable to us as political scientists with our problem of the relations among the subfields. Thus, in our diversity we should not ignore one another but should engage each other in a competitive spirit, recognizing that we could be wrong but always ready to push to higher levels the potentials of our subspecializations. Diversity based only on toleration will not produce progress. What is needed is engaged diversity and intense interaction if we are to give new vitality and a greater sense of direction and purpose to political science. Events in the world have presented us with a great challenge and we are the discipline with a responsibility to enlighten the world about the current crisis of authoritarianism.

Notes

This essay is the presidential address presented on 2 September 1989 at the 85th annual meeting of the American Political Science Association in Atlanta.

1. Farr (1988), in reviewing Finifter 1983 and Weisberg 1986, documents the problems of pluralism and collective identity of political science by an extensive review of the literature, including the APSA presidential addresses of Woodrow Wilson in 1911, Charles Merriam in 1921, Charles Beard in 1927, Harold Lasswell in 1956, David Truman in 1965, David Easton in 1969, Emmette Redford in 1961, Heinz Eulau in 1973, Leon Epstein in 1980, and Charles Lindblom in 1982, and such other reports on the discipline as Somit and Tanenhaus 1967, Crick 1959, Ricci 1984, and Seidelman and Harpham 1985.

2. As a discipline, political science often seems to want to have its cake and eat it too, in the sense of wanting to be simultaneously both scientific and humanistic. In our scientific mode we like to pretend that we are engaged in producing cumulative knowledge. Thus, we act as though the latest "paradigm" is also the best. But of course this is not true, especially with respect to humanistic knowledge—for as

Robert Lowell once observed, "The arts do not progress but move along by surges and sags" (Cohen 1989, 3). The practice of claiming that the latest fashion has demolished all that has gone before constitutes not only a bizarre misreading of the differences between scientific and humanistic knowledge but also a violation of the civilities of science. With embarrassing frequency scholars will seek to advertise the importance of what they call their new "paradigm" by giving the back of the hand to all previous efforts, declaring them to be outmoded or not useful." Valerie Bunce (1981, xii, quoted in Harding 1988) is not far off the mark in observing that "while physicists stand on each other's shoulders, political scientists stand on each other's faces." In physics any attentive freshman will learn more about the nature of the atom than did Maxwell, Milliken, Tompson—whose theories have all been proven flawed; but these pioneers are still revered immortals in physics (Morris 1987).

Although the signs are ambiguous, there are hints, from some of the subfields, of growing dissatisfaction in both the scientific and the humanistic wings of the discipline with the sharp distinction that Kant helped to establish in formulating the mind-matter dichotomy. Daniel Bell (1981) has raised the possibility that all of the social sciences may be reaching a point of exhaustion in our separate searches for scientific invariances and for humanistic interpretations and that progress will call for a new sense of unity. This would be the unity of the learned, the bond that should hold the academy together. But unfortunately, our diversity of subfields makes unity exceedingly unlikely.

3. We are fortunate in political science that the swings between these poles has not produced the kind of intense ideological conflicts that have erupted in some of the other social sciences. In anthropology, for example, there has been near paralysis among younger scholars who agonize over whether any knowledge is authoritative or even possible and who are filled with fears that any concepts they might use in ethnographic research could constitute a form of Western cultural imperialism masquerading as objectivity. One anthropologist speaking of this "crisis of faith" has suggested that his discipline is "in need of a farm subsidy program for Western intellectuals: to avoid flooding the market with ideas, pay them not to think" (Richard A. Shweder, "The How of the Word," *New York Times Book Review*, 28 February 1988).

The epistemological doubts that have produced this crisis of faith among anthropologists stem in large measure, according to Earnest Gellner, from the antiintellectual, "thick description" school of hermeneutics, which sees the use even of clarity and logic in analysis as a dangerous form of Western ethnocentrism. As Gellner notes, "It is not obvious to me that, because the world is a diverse, complex, and tortured place, which it is, that only cumbersome and ambiguous sentences can do it justice, and that clarity is some kind of intellectual treason" (Gellner 1988, p. 302).

4. An equally conspicuous trend toward economic liberalization and greater respect for the merits of market forces has accompanied these political upheavals and even been a driving force propelling them forward. Ideologues of the Right should, however, be cautious about proclaiming the victory of capitalism and democracy. It is precisely the relationship between economics and politics that needs to be reexamined in the light of current developments and in a spirit that is as neutral of old ideologies as is possible in a scholarly environment.

5. The most detailed studies of pre-World War II foreign investments in China set the figure at about 3.4 billion dollars. See Carl F. Remer 1933 and Chi-ming Hou 1965. Before Tiananmen U.S. involvement in the Chinese economy was about 3.5 billion dollars.

6. At the July 1989 Round Table meeting in Moscow of a delegation from the APSA and representatives of the Soviet Political Science Association (SPSA) Georgii Shakhnazarov, president of SPSA and member of the Central Committee of the Soviet Communist Party, declared that political reforms called for the development of a kind of "politology" (as they call political science) that would be "deideologized" and "value-free and objective." In short, out with Lukacs and Gransci and in with Max Weber.

7. Theorists in both the Chinese Academy of Social Sciences and some of China's leading universities briefly came to the support of the official orthodoxy in 1988 by trying to develop what they called the theory of "neo" or "new" authoritarianism and which they thought might legitimize China's economic "reforms" under conditions of political restrictions. They argued that China could and should follow the examples of Korea, Taiwan, Singapore, and Hong Kong, in which rapid economic development took place under authoritarian governments that sought progress and were not backward looking, as were traditional, or "old" authoritarianism. They even sought to support their analysis by citing U.S. scholars who in the 1950s and 1960s suggested that dictatorial systems had some advantages in modernizing. What the Chinese scholars seemed to misunderstand was that Samuel P. Huntington and the members of the Social Science Research Council Committee on Comparative Politics, in stressing the importance of the institutionalization of government authority for political development, did not have in mind the arbitrary ways of China's personalized government and its rule by men rather than by law.

8. At that same Moscow Round Table meeting, a Soviet academician stated, "We Russians are backward because we have always clung to illusions. For a long time we lived with the illusion that we were at

the forefront of history because that was what our Marxism-Leninism taught us, but we actually lagged far behind the rest of the industrial world. And now we are living with the illusion that *perestroika* will work."

9. Alasdair MacIntyre (1978) has drawn attention to the problem caused by the uniqueness of cultures by raising the shocking question whether a science of comparative politics is theoretically possible. He argues that because political institutions and political cultures are inseparably interrelated and since cultures are by their nature unique, this means that at a fundamental level all political systems are idiosyncratic. MacIntyre seeks to get around his problem by stressing, in much the same manner as Karl Popper, the role that purpose or human will plays in providing a degree of order and predictability in human affairs. Comparison of different systems becomes possible when they share the will to achieve the same ends—such as modernization and development. There is some validity to MacIntyre's position, as can be seen from the fact that comparisons about economic development are easier to make because the goals of such policies are less ambiguous and more universally shared than is the case with political development in which the guiding purpose can vary from country to country according to sense of national identity. My case is arguably similar to MacIntyre's solution, in that the common need for all countries to respond to the universal character of the world culture provides a political equivalent to the goal of economic development. Yet I am not prepared to accept MacIntyre's view that culture belongs totally to the domain of "thick description." Basic to cultures are the generalizable characteristics of human nature, especially the universal features basic to personality development in the socialization process. As Henry Murry once remarked about individuals, "Every man is in certain respects like all other men, like some other men, like no other man" (Kluckhorn and Murry 1953, 53). It is the same with nations.

10. We have to acknowledge as students of government the somewhat embarrassing fact that the main obstacle to human progress is all too often stubbornness of political individuals and the follies of governments. Yet take heart: The best and the brightest of the young Cambridge economists, who are queued up for future Nobel Prizes, now say that the popular idea of the early 1980s, that of perfect competition of markets, is now déclassé and that at times there is a need for (and they say this with a straight face) the "rationality of governmental policy" to solve problems caused by market failures. As political scientists we can be bemused by this charming faith in the rationality of the political process.

11. The World Bank reports that during the 1980s the newly industrialized countries of Asia reinvested nearly one-third of their total output, whereas in Latin America and Africa the comparable figures were only 14% and 12%. Investment in Latin America in the maintenance of highways, water systems, electric power, and telephone systems was so low that by the mid-1980s deterioration had brought all these below their 1950s levels; while subsidies fueled inflation, which in Brazil has exceeded the 1923 hyperinflation in Germany. The story of China is also that of a "transferral economy," as the state has been unable to maintain the appropriate infrastructure of power, transportation, and communications for a growing economy because over 60% of the resources have been devoted to subsidies that grossly distort prices, thereby encouraging "irrational" investments.

12. In the year preceding the student demonstrations in Tiananmen Square Chinese intellectuals were engaged in some profound soul searching about the reasons for China's failures at modernization. The Chinese public was also caught up in wondering why China, which had once been so great as to be the "center of the world," was now so backward compared not just to the West and Japan but also to its immediate neighbors. The level of self-criticism reached a high point with the publishing of several critical books and the showing on national television of a six-part series called the *River Eulogy*, which graphically portrayed the sorrow of Chinese backwardness, the shame of its cultural traditions, and the need to become more a part of the world. The three young men who wrote and produced the series were quietly arrested in the repression that followed the Tiananmen Massacre, and we do not at this time know their fates.

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THE INTERNATIONAL TRADE COMMISSION AND THE POLITICS OF PROTECTIONISM

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I analyze the governmental regulation of internationally traded goods produced by U.S. industries. General theories of regulation—most notably "capture" theories and the theory of "congressional dominance"—are used to analyze the decision-making behavior of the U.S. International Trade Commission, which plays a major role in approving and providing tariffs, quotas, and various types of nontariff trade barriers sought by these industries. Unlike previous studies, this one simultaneously accounts for both the supply and demand sides of trade regulation. This work seeks to predict, on a basis of domestic politics, the factors that affect the demand for, and supply of, trade protection for U.S. industries. The methodology consists of applying a nested logit framework to capture the decision behavior of the International Trade Commission and industries simultaneously. The analysis shows that industries do appear to self-select themselves in applying for protection from the International Trade Commission. In light of these findings, it appears that trade protection is subject to domestic political forces similar to those affecting other regulatory policy areas.

Industries in the United States enjoy varying degrees of protection from foreign competition. While economic reasons may exist to justify some of these differences in protection, most economists and political scientists agree that one needs to look at the politics behind protective legislation to understand industry-specific differences in government assistance. My purpose here is to try to explain the varying levels of protection across industries by focusing on factors that affect both the supply of, and demand for, the regulation of trade. What circumstances lead industries to request protection and what factors affect the government's decision to supply that protection or not? Both industries and the government presumably have incentives to pursue utility-maximizing courses of action. On the demand side, when an industry seeks a higher tariff, the benefits from that tariff presumably outweigh the costs of applying and lobbying for protection. On the supply side, when the government chooses to protect an industry, the political benefits in terms of votes or contributions presumably exceed the loss of support from those harmed by the policy.

Given the incentives of the actors, I seek to predict, on the basis of domestic politics, the factors that explain industry and government decisions on trade matters. Why, for example, did the electric golf cart industry get higher tariffs in 1976 when the hand tool industry was turned down? In 1983, frozen orange juice makers got protection, but the canned mushroom industry was unsuccessful.

What incentives did these industries have to apply for higher tariffs, and on what basis were golf carts and orange juice chosen for protection while hand tools and mushrooms were not? In other words, what factors best explain the actions of interest groups and the decisions of regulators? People have studied tariff policies for decades; yet existing studies, containing conflicting results and diverse methodologies, have left unanswered questions and offer results that can be challenged. In pursuing answers to these questions, I hope to develop not only a better understanding of how trade policy is made in the United States but also a better understanding of government regulation in general.

Though a number of studies have recently been done on the International Trade Commission (ITC), the conclusions that have been drawn in these works are questionable. One major problem of the literature on both the ITC and the regulation of trade in general has been a failure of scholars to account for both the supply and the demand sides of the regulatory issue. For example, by focusing only on the supply of regulation, that is, the decision of the ITC to grant or deny petitioners protection, scholars have ignored the possibility of a pool of applicants having common, unobserved characteristics that lead them to apply in the first place. This study makes an attempt to test for this potential problem of industries self-selecting themselves when seeking regulation.

The first section includes a discussion of the ITC and its role in the making of trade policy. The next section reviews the existing literature on trade regulation, particularly studies on the ITC. The third section explores general theories of regulation, especially the capture theories and models of pressure group competition, in order to identify testable hypotheses about trade regulation. In the fourth section, an econometric model is developed for testing these hypotheses in relation to the theories just developed, the data to be used are discussed, and the estimations and results are presented. Finally, I summarize the findings and discuss the important implications of this work to the study of trade and regulation in general.

The Role of the ITC

The power to regulate international trade, granted to Congress under the Constitution, has evolved into a complicated set of procedures involving all branches of government. In the eighteenth and nineteenth centuries, setting tariffs was a major activity of Congress. However, by the twentieth century, it was evident that Congress was moving away from any direct activities in the regulation of trade. In 1934 under the Trade Agreements Act, Congress delegated to the president the power to raise and lower tariffs. Then, by the late 1940s and early 1950s, the United States Tariff Commission (the future ITC), created in 1916 as a fact-finding agency, acquired greater authority from Congress in the decision-making process of trade regulation. A vast majority of the casework on trade protection today is carried out by the ITC.

The ITC gathers data and conducts investigations in a number of areas. Three of the six major statutes involving the ITC allow industries to seek direct relief from foreign competition. These are the escape clause investigations (secs. 201, 203), antidumping investigations (secs. 733, 735), and countervailing duty investigations (secs. 703, 705).[1] By statute, the ITC conducts these investigations on its own initiative, at the request of a firm, an industry representative, a union, or group of workers; and, for escape clause cases, at the request of Congress or the president. All three of these types of investigation are means by which industries can seek relief from foreign imports and in all three cases, the role of the ITC is to determine injury or threat of injury to the domestic firm.

Relief can take the form of an increase in tariff duties, quantitative restrictions (quotas), negotiated agreements, or adjustment assistance in loans, tax breaks, or the like to the petitioners. ITC decisions on escape clause cases are subject to presidential veto, but can be overturned by a joint resolution (a majority vote in each House) in Congress. This joint resolution is subject to a presidential veto for reasons of national interest, which Congress can override with a two-thirds vote in both chambers. A number of years after relief has been imposed (as specified in the final decision, but at least two), the ITC conducts a follow-up investigation to determine whether the elimination of that relief would cause harm in the future to the industry in question. Escape clause proceedings have been an important part of trade legislation since Congress began delegating its authority to make tariff adjustments.

ITC investigations on antidumping and countervailing duty cases are carried out in conjunction with investigations by the Commerce Department (formerly by the Treasury Department). Dumping entails selling goods to a foreign country at less than the home market cost of production. A countervailing duty is a fee imposed on imports to counter subsidization that takes place in the foreign market. While the Commerce Department—in particular the International Trade Administration (ITA)—determines the existence of dumping or subsidization, the final decision on both types of cases lies with the ITC in determining whether the dumping or subsidization is injuring the industry or threatening the industry with injury.

All three of these types of investigations are avenues through which industries can petition the government for protection; and in all cases, the role of the ITC is to determine whether or not injury has occurred or is threatening the industry. The goods for which industries seek protection through the ITC under these three statutes represent a wide variety of products—from clothespins and plastic animal identification tags to steel products and automobiles. The process is often long and seemingly costly;[2] yet many industries and even small individual firms find the effort worthwhile, as is apparent from the number and nature of yearly petitions. Yet how much do we or firms know about their probability of success in seeking protection or the factors involved in the decisions of policy makers? The next section reviews developments in the literature on trade and the ITC in trying to understand the regulation of trade in the United States.

Two Approaches to Protectionism

Dominating the literature are two approaches that offer explanations for the historic development and present degree of protectionism in the United States. One theory postulates that the structure of the nation-state and the interdependence of nations are the key to understanding trade policies. Nations pursue a variety of interests, and their degree of openness reflects overall foreign policy goals and their position of power in the world (see, e.g., Keohane 1980; Krasner 1978; Rosecrance 1986). A nation pursues protection when its international power is threatened. The second theory is an interest group approach that focuses on the ability of pressure groups to influence policy outcomes. Here the earliest studies related to trade policy are those of Bauer, Poole, and Dexter (1963) and Schattschneider (1935). Tariff policy was explained as the result of domestic political influences on congressional decision making, but these analyses ignore the institutional context and the rules governing the policy makers themselves. An in-depth look at both bodies of literature reveals a large number of empirical studies aimed at predicting the factors that explain U.S. trade policies.

Edward Ray (1987) presents a model of trade policy that combines these perspectives. He claims that government actions are guided by both national policy goals and special interest groups. When both preferences are for freer trade, government policy will favor free trade. But when these forces clash, with a national preference for free trade but special interest preferences for more protection, the result will be ambiguous policies. This phenomenon, Ray claims, will lead to a trend toward nontariff barriers (NTBs) to trade rather than increased tariffs. Special interest groups incapable of getting tariff protection from the government will turn to NTBs, which are industry-specific and circumvent the normal trade barrier channels that may be opposed to protection because of national policy concerns. Ray claims this has been the trend in recent years (Ray 1987, 288–92). This notion may have important implications for the role the ITC plays in providing a means for industries to seek trade protection as an alternative to the more national, publicly visible, international tariff negotiations.

Also worthy of mention is the ongoing debate in much of the early empirical literature on the relationship between labor intensity and U.S. tariff rates. The main concern was over the question of whether U.S. trade policies tended to protect labor in manufacturing industries. These works rested on the intuition of the Stolper-Samuelson theorem (1941) that protectionism tends to favor a nation's scarce factor of production used relatively intensively by import-sensitive industries that are less competitive internationally. Studies (e.g., Vaccara 1960) found evidence that tariff rates in the United States vary positively with labor intensity. A number of studies that followed cast doubts on Vaccara's findings. For example, Cheh (1976) and Basevi (1966) found evidence refuting the claim that the United States protects labor-intensive industries. In particular, by considering both tariff and nontariff rates of protection for 1964, Cheh found no significant relationship between protection and the labor intensity of U.S. manufacturing industries; while Basevi claimed that including the tariffs on the inputs of manufactured goods reversed the relation between protection and labor intensity. Some modern empirical studies, however, suggest that unskilled labor-intensive industries in the United States do receive more protection (see, e.g., Magee 1973, Clark 1980). By dividing labor into groups of skilled and unskilled workers, they found that protectionism is biased toward unskilled labor-intensive goods. But this has been an ongoing debate in the literature.

Of greater concern here is the vast literature on industry-specific variations in levels of import protection. A large number of cross-industry studies attempted to identify more general determinants of the regulation of trade. A majority of this literature focused on various characteristics of the demand for protection or the supply of protectionist measures or both (see Caves 1976; Godek 1985; Marvel and Ray 1983; Pincus 1975; Pugel and Walters 1985; Ray 1981). On the demand side, firms, industries, and workers, desiring to maximize their earnings or minimize their losses and economic hardship, seek relief from import competition through bureaucratic and legislative processes. On the supply side, protection is awarded by the ITC, the Commerce Department, Congress, and the executive, who determine by political or economic criteria which industries will be granted import relief. This general body of literature is composed of scholars' attempts to predict equilibrium tariff rates with a variety of supply- and demand-side variables.

A more specialized body of literature

includes micro studies of trade regulation that deal specifically with the actual decision making of the ITC. This literature includes the works of Baldwin (1985), Finger, Hall, and Nelson (1982), Goldstein (1986), Lenway (1985), Shughart and Tollison (1985), and Takacs (1981). A common theme in many of these works is the insignificance of pressure politics on policy outcomes. These authors would have us believe that domestic politics play little or no role in explaining protectionist decisions today. In other words, though interest groups pressure legislators for protection, policy makers do not respond to their demands.

For example, Goldstein (1986) examines ITC actively to see whether the agency has become more or less protectionist over time. She claims scholars have focused on the demand for protection, not on the supply or the actual amount that has been imposed. Goldstein looks at the acceptance rate of ITC petitions from the 1950s to the present and concludes that it has not changed significantly over time. She concludes that policy is determined by the state's central decision makers, whose liberal beliefs and attitudes restrain their actions and prevent rampant protectionism. Lenway (1985) comes to a conclusion similar to Goldstein's, namely, that pressure groups have little influence on trade policy. Her claim is that the General Agreement on Trade and Tariffs is the constraining force for U.S. policy makers.

Though Goldstein finds that the acceptance rate of petitions for protection has not increased over time, and Lenway finds that industries do not always get what they ask for, it is important to point out that this does not imply that protectionist pressures have not had an effect on policy makers. Some industries have been protected as the result of ITC rulings, and politics has surely played a role in determining which industries were chosen and the degree of protection that they were offered. Perhaps more interesting is the question of which petitions have been successful; that is, To whom have the protectionist favors been granted?

The broad body of literature presented here represents a significant attempt by scholars to explain the regulation of trade. Many factors are identified to explain the supply of, and demand for, protection, but problems are apparent in much of the literature. There are few comprehensive empirical studies, and it is frequently unclear whether the authors are measuring supply or demand or some convoluted combination of the two. Secondly, the narrow focus of some of the studies leads to contestable conclusions. Finally, the exclusion of political institutions in these models is pervasive.

The studies on the ITC, in particular, draw conclusions about the supply of regulation by the commission without properly accounting for industry demand. Ignoring an industry's decision of whether or not to file an application for protection may result in a self-selection bias in the analysis of the supply of regulation. Self-selection will be a problem if an industry's decision to apply (or not) is affected by its perception of the likelihood of a positive ITC ruling.

I draw heavily on this body of trade literature in an attempt to improve our understanding of protectionism. By looking at the behavior and incentives of both policy makers and industries, I seek to predict on the basis of domestic politics the factors that best explain the demand for, and supply of, regulation; that is, which industries are more likely to seek protection and when the ITC is more likely to grant them that protection. My study revolves around two well-known bodies of literature on pressure group competition for regulation. I identify several hypotheses developed from these approaches and then test these hypotheses using data on ITC decisions, while accounting for the problem of self-selection.

Theoretical Foundations

It is assumed that industries seek regulation in order to increase their wealth. Barriers to trade not only create economic rents for the industries but also provide revenue to the government that can be used to subsidize specific industries and workers being hurt by foreign competition. The question, then, is why are some industries protected when others are not?

In exploring the answer to this question, one needs to look at the incentives of the actors in the regulatory process. Which factors explain industries' incentives to seek protection and which factors explain the regulatory agency's incentives to supply that protection? From existing trade and regulation literature, these factors will be identified and quantified in order to test the power of existing theories of regulation in explaining protectionism in the United States.

In the broad field of competitive market regulation, two well-known approaches have dominated the literature in recent years. The first is best known as the "capture theory" approach developed first by Stigler (1971) and further refined by others, such as Peltzman (1976) and Becker (1983). The essence of the capture theories is that political groups compete amongst themselves for political influence and wealth by lobbying, and applying pressure on, the politicians or regulators. Regulators or politicians grant regulation to interest groups in such a way as to maximize their political support. These models then posit a demand for, and supply of, regulation.

The second approach, known as the theory of "congressional dominance," includes the works of Shepsle and Weingast (Shepsle 1979; Weingast 1981, 1984; Shepsle and Weingast 1984), Weingast and Moran (1983), and others. The bureaucracy here is no longer captured by interest groups. Other institutions, particularly Congress, are given more attention. The basic tenets of this set of theories is that Congress controls the bureaucracy through such things as oversight committees and control of the purse strings. Congress is thus a key institution in the distribution of regulatory benefits, a feature the capture theories lack. Here, again, there is a supply and demand for regulation. Interest groups compete for benefits, which are supplied by the regulatory agency; but now the agency is controlled by the will of Congress. Members of Congress favor agencies that serve their constituencies and oppose those that do not by preventing career advancements, curtailing budget allocations, and controlling the appointment-reappointment process.

These two approaches to the study of regulation comprise a literature that develops the strategies and goals of the actors in the regulatory process. In general, interest groups form to pressure politicians for regulation when they believe that they can derive economic benefits larger than their costs of demanding that regulation. On the supply side, regulators, faced with the problem of choosing where to allocate the benefits among the many competing interest groups, seek to maximize their political support. I use this literature as a basis for testing both the political actions of regulators and the decisions of industries desiring protection.

From existing trade and regulatory literature, several factors for each of the supply and demand sides of regulation can be identified, which, respectively, may influence the actions of the regulators and industries.

Supply

On the supply side, general theories of regulation (see, e.g., the capture theories of Becker [1983], Peltzman [1976], and Stigler [1971]) predict that regulators seek to maximize their political support. To accomplish this goal, the regulator needs to satisfy key interest groups that will provide that support. In other words, the regulator will seek to satisfy politically strong groups in society at the expense of politically weak groups. The political strength of interest groups, particularly of industries seeking trade regulation, depends on several key factors that are readily identifiable in the regulation literature.

According to regulatory theories, particularly the Becker model, one factor that should affect the supply of regulation is the efficiency of policy alternatives; that is, politicians decide both the nature of the regulation and which industries are granted regulation on the basis of what offers the least costs and greatest benefits to society while maximizing their own political support. Regulation will be granted to industries where the deadweight costs of the policy are lowest and the subsidies are largest. Thus, the group that finally receives support is chosen because such a policy is the most efficient in terms of costs and benefits. This comprises the first hypothesis to be tested. A good measure of the deadweight costs of a tariff on a certain good is the elasticity of demand for that good. In the long run, assuming perfectly elastic supply, more inelastically demanded goods will have a lower deadweight loss for a given tariff rate. Tariffs on these goods are more efficient. Thus, industries that produce more inelastically demanded goods should be more likely to receive regulation.

The political significance of the distribution of costs and benefits is emphasized in the regulation theories developed by Stigler (1971), Peltzman (1976), and Becker (1983), as well as the theory of collective action developed by Olson (1965) and other studies on trade policy, including works by Salamon and Siegfried (1977), Aggarwal (1987), Finger, Hall, and Nelson (1982), and Marvel and Ray (1983). Benefits distributed to a single industry with costs dispersed over a large number of taxpayers are not likely to be opposed. This is because taxpayers have little incentive to organize and oppose small dispersed costs, so regulators are more likely to supply benefits when no particular segment of the public sector bears all of the costs; that is, industries are more likely to be granted regulation as long as there is a large tax-paying group.

A second factor, then, that may affect an industry's ability to influence regulators is its size. According to Olson, it has been observed that groups that are small relative to any opposing group are more effective at organizing and taking action (Olson 1965, 52). Since industries are small relative to the tax-paying groups in society, they should be more effective at pressuring for higher tariffs than taxpayers should be at opposing such policies. However, successful pressuring of politicians also necessarily requires an ability to command power and influence. Thus, larger industries, which are generally more powerful and persuasive, should be more effective than smaller ones at influencing the political system and obtaining desired policies. Also, according to Becker (1983; see also Peltzman 1976), small groups may not be able to take advantage of economies of scale in the production of pressure (p. 395). A counterargument has been proposed in the literature (see, e.g., Salamon and Siegfried 1977) that the free rider problem for large industries would discourage their application of pressure, but it is hypothesized here that larger industries will have larger resources with which to influence decision makers. To test this second hypothesis that size affects policy outcomes, a good proxy for the size of an industry is its total employment.[3] The higher an industry's employment, the greater should be its ability to press for and obtain regulation.

Related to this notion is the political significance of group organization. The degree of organization of an interest group will affect the supply of, as well as the demand for, regulation. This is because the costs of making decisions and coordinating actions are much smaller when a group is more organized. Thus, the group will be more efficient at producing pressure and affecting policy outcomes, so industries that are in some way more organized will have more political strength and thus will be more likely to affect the decisions of politicians who supply the regulation. Several variables were used as proxies for measures of the degree of organization of an industry. Concentration and unionization are two good measures for the degree of organization. In general, industries with higher degrees of concentration and unionization should be more organized and better at pressuring policy makers. Higher concentration in an industry implies fewer free rider problems and more opportunities for reaching a consensus, thus a greater degree of organization. Unionization (i.e., involvement of a union in a petition) implies organization and an opportunity for collective action. Thus, union support should positively affect an applicant's ability to pressure policy makers. A third measure of the degree of organization of an interest group is based on whether there is a single-firm or multifirm petition. If the industry pressure group is made up of a number of firms applying together as a group for protection, they have presumably overcome some of the costs and barriers to organization. So the higher an industry's degree of organization, measured in terms of concentration, unionization, and type of petitioner, the more successful it will be at affecting policy outcomes.

Another possible affect on a politician's decision to supply trade regulation may be due to political pressure at a macro or national level. Takacs (1981), for example, focuses on cyclical national economic trends in explaining ITC behavior. A general slump in the economy, which results in such problems as high unemployment, inflation, large deficits, or the like, may put pressure on the regulators to take some action to improve the economic conditions of the public, especially the more outspoken groups of society. So national economic trends like the U.S. trade deficit and the unemployment rate were used as proxies for national trends that might affect the supply of regulation.

In line with the "congressional dominance" literature, several variables were identified to measure congressional influence over ITC decisions. Because the ITC is an agency created and both funded and reauthorized annually by Congress, this should necessarily entail a look at this institution's possible influence on the behavior of the commission. In studies of "congressional dominance," the influence of Congress on agency decisions has typically been measured by such things as the liberal-conservative voting scores of members of Congress, constructed by Americans for Democratic Action (ADA scores), the ADA ratings of the relevant committee(s) and its chair, or the political party make-up of the committee or committees that oversee the agency. Also, the party of the president has been used to account for presidential influence over policy outcomes of agencies.

This type of analysis, however, presents problems in this work for two important reasons. First, because of the nature of trade legislation, it is difficult, if not impossible, to determine how a given member of Congress or the president stands on the issue of granting protection merely by considering political party affiliations or ADA ratings. Though generalizations have been made, neither party identification nor liberal-conservative measures of voting behavior seem to accurately predict whether or not protection or free trade will be the preferred outcome. Historically, because they face a national constituency, both Democratic and Republican presidents have favored

—or at least purported to favor—free trade. Furthermore, members of Congress of both parties or political leanings have been on both sides of the protection–free trade issue. For example, in 1982 the Caribbean Basin Initiative bill (H.R. 7397) came before the floor of the House of Representatives for a vote. Several amendments were allowed under the rule, including one by Richard Gephart (D-Missouri) to protect tuna and one by Larry Hopkins (R-Kentucky) to protect tobacco (*Congressional Quarterly Almanac* 38 (1982): 151–56). Though these amendments can be interpreted as mere position-taking stances on the part of these representatives for the sake of satisfying constituent requests or demands, instances of such behavior abound. Thus, one might question whether other factors besides party affiliation might not be better indicators of where members of Congress stand on trade issues. The difficulty of determining how they stand on the issue of protectionism by their party identification is even more pronounced if one looks at congressional behavior over time. Republicans were historically more protectionist than Democrats, but factors such as the pressures of rising unemployment have blurred this distinction. In fact, it appears that Democrats take a protectionist stance today more often than Republicans do.

Secondly, because many changes have been made in the legislation governing the ITC since its creation, it is very difficult to undertake a time series study that allows for variation in the make-up of relevant congressional committees and changes in committee chairs or presidents. Since the rules under which the ITC operates have been fairly stable since 1974, the period from 1975 to 1984 was chosen for this study. Although industry-specific tariffs were changed during this time, by party identification the Ways and Means Committee membership has only varied by a few percentage points from one year to the next. The percentage of Democrats on the committee had a range of only 65%–67% over the 10-year time period covered in this study. Also, the chair was fairly stable, changing only once in the 10-year period; the change occurred in 1981 when the chair switched from Democrat Al Ullman to Democrat Dan Rostenkowski. Because of this stability, it is difficult to draw conclusions about what effects, if any, party identification might have on trade policy outcomes. But at the same time, extending the time period of the study would not solve the problem of member stability because from 1962 to 1974 more stringent requirements for granting protection were governing the ITC. This different set of rules is probably responsible for severely restricting the number of applications that were received by the commission in the 1960s and early 1970s. Under a different set of rules, a different set of criteria may have been used by industries in deciding whether or not to file an application. Thus, pooling data from after 1974 with data prior to that year would likely lead to erroneous conclusions.

Because of the difficulties of dividing Democrats and Republicans (or liberals and conservatives) on the issue of protection versus free trade and because of the problems that emerge in studying ITC behavior over longer time periods, the traditional means of measuring congressional or presidential influence could not be effectively used in this work. Accordingly, incorporating political institutions into a study of the ITC requires a new or different focus and means of accounting for a possible relationship between Congress and the behavior of the regulators on the commission.

One possibility, not explored in the literature, is to look more closely at the geographic location of industries in relation to congressional power. The question is, Are industries that have firms located in the districts of powerful or influential members of Congress on the relevant committees more likely to be granted relief by the ITC than those that have no such representation? Since the Ways and Means Committee and the trade subcommittee have jurisdiction over trade matters in the House of Representatives, one good measure of congressional influence is a measure of representation, by industry, on these committees. By looking at geographic concentration and location of industries, perhaps we can better understand and predict whether or not the ITC will respond favorably to an industry's demands.[4]

Congressional representation is accounted for in this study by the creation of eight variables. Each industry has operations in a number of congressional districts. The number of times those operations (with an employment of 250 persons or more) existed in districts represented by members of the Ways and Means Committee or trade subcommittee was counted and recorded. These variables were further divided by party. Also, four dummy variables were used for representation by the chair or ranking member of either Ways and Means or the trade subcommittee. For example, Democrat William Green from the Third District of Pennsylvania was the trade subcommittee chair from 1975 to 1976. A variety of industries had operations employing over 250 people in his district. These included milk products (standard industrial classification [SIC] no. 2026), various textile products (SIC 2253, 2269, 2292, 2295), paperboard box products (SIC 2642, 2651, 2652, 2563), plastics (SIC 3079), fabricated metal products (SIC 3442, 3443, 3444), and industrial machinery (SIC 3544, 3559, 3566, 3573), among others. These industries were coded 1 for the years in which Green served as chair, 0 otherwise. In this way the influence of committee oversight on ITC decisions is measured while avoiding the problem of mixed liberal-conservative responses on trade matters.

Though this study is congressional centered, the president was not ignored. A dummy for party identification of the president was included to capture possible presidential effects. However, because the president's constituency is not a source of variance in the same way that a member of Congress's constituency is, his influence on which industries are protected is not likely to be as observable here, and the results indeed bear this out. Presidential influence is more likely to be observable in explaining decisions made by the ITA, which has the task of determining dumping or subsidization on the part of the foreign firms, not injury to domestic firms. This issue is being further explored.

Besides these political factors, the supply for protection may be affected by apparent or genuine need for relief on the part of the industry. An industry that is suffering from rising unemployment, loss in domestic market share due to foreign imports, and low use of plant capacity should be more likely to get relief. Also, an industry with a low existing tariff rate should be more successful. However, the ITC may not have an "ideal" rate in mind, and thus may not take the existing rate into account. These variables are used to capture economic hardship and need in the industry.

Demand

Similarly, certain factors exist that may help to determine whether or not an industry will demand regulation. With rational actors of minimal sophistication, any factor influencing supply can be anticipated and thus influence demand. Because of the "two-tiered" structure to the system of regulation, political authorities like Congress or the president may attempt to influence the behavior of the commission, which probably does not directly affect the behavior of firms in deciding whether or not to file a petition for protection (Moe 1985, 1100). Rather, firms observe the behavior of the ITC and assess their probability of success and their ability to organize and file a petition. This problem is dealt with in the econometric model developed next. But in addition, on the demand side firms observe their economic status and need for help.

In terms of their economic hardships, loss of market share may drive an industry to seek some form of regulation that will raise profits. A good measure of this trend is the percentage change in an industry's domestic market share due to competition from imports. In general, industries with a relatively large loss in market share to imports should be more likely to demand regulation. Also, an industry that is forced to cut costs and lay off employees should be more likely to apply for protection. Pressure from workers or unions may drive an industry to seek regulation when employment is dropping in the industry. This trend is measured by the percentage change in employment in each industry.

Another factor affecting an industry's demand for regulation may be the industry's prior level of regulation. A highly protected industry is not only less likely to get protection but is also less likely to request or pressure for additional regulation than a relatively unprotected industry, since the costs of acquiring additional regulation would likely exceed the benefits. In other words, there are increasing costs and diminishing returns to acquiring additional regulation. Thus, an industry's existing tariff rate should also be a good measure of its likelihood of seeking protection.

Finally, the degree of organization in an industry may affect its decision to pressure for regulation. A more-organized interest group will be better at pressuring for regulatory benefits and securing desired policy outcomes. Also, an industry that is highly organized and controls free riding will face fewer costs and reap larger benefits from pursuing regulation. The degree of organization can be measured by the concentration ratio in an industry. Higher concentration implies fewer free riders and lower costs of organization, thus a greater likelihood that an industry will demand regulation.

Data and Econometrics

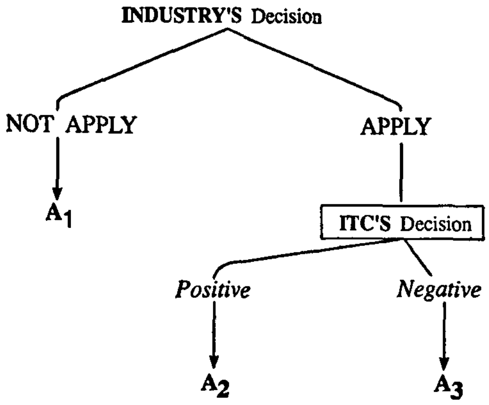
The data on regulatory decisions by the ITC in this study cover a period of a little more than 10 years, from the passage of the 1974 Trade Act to the beginning of 1985. These years were chosen because the rules under which the ITC operated prior to 1974 were significantly different, enough to preclude pooling data from before 1974 with data from after 1974. In this period, the rules governing the behavior of the ITC were fairly stable.[5] The data include all ITC cases considered under the escape clause and the antidumping and countervailing duty investigations as discussed earlier, since the inclusion of all three types of cases is crucial to measuring the demand for regulation from the ITC.[6]

On the supply side, there were 616 investigations conducted by the ITC between 1975 and 1984. Of these 616 cases, there were 314 antidumping cases, 231 countervailing duty cases, and 71 escape clause cases. These were categorized by the four-digit SIC code corresponding to each petition. At the four-digit SIC level, there were 290 industry cases. (This number is smaller than the total number of cases because some four-digit industries filed more than one petition, and is further reduced in the estimations to 205 due to missing data on various exogenous variables. If one industry in the four-digit category filed a petition, it was counted as a petition by that four-digit category.) The outcome of the industry petitions is the dependent variable. A value of one is assigned to industries if any case received some form of relief through ITC action in a given year and zero to those that were denied relief or protection. About 40% of the industries that filed petitions in these years were granted some form of regulatory relief.

On the demand side, there are 425 four-digit SIC manufacturing industries included in this study with data for the same period, 1975–1984. These are all industries that compete on some level with foreign imports for a share in the U.S. market. A value of one is assigned in cases where at least one petition is filed for any industry in a given year and zero to cases where no petitions are filed. Industries filed a petition with the ITC in only about 7% (290) of the 4,250 entries across the 10 years. (Again, the number of observations was reduced in the estimations to only 2,903 cases because of missing data on various exogenous variables.) However, it should be noted that 133 of the 425 industries (31%) overall actually did file petitions at some time during this 10-year period, so the sample does represent a substantial number of industries.

All of the data used in this study are coded at the four-digit SIC level except the elasticities, where three-digit level data are used instead, with entries repeated for each corresponding four-digit code. Details on the independent variables used to explain the supply of, and demand for, the regulation of international trade are provided in Appendix A.

Figure 1. Two-Stage Decision Process



A nested logit model (McFadden 1978) is applied to the study of the demand for, and supply of, trade regulation to determine whether industries base their decisions to apply on their perception of the expected utility of getting protection. The demanders (industries) face the binary choice of whether or not to undergo the costs of applying and pressuring for trade regulation. The supplier, the ITC, makes the binary decision of whether or not to grant regulatory benefits to each of the applicants. This study aims to determine whether self-selection is a problem in predicting the probability of an industry getting protection; that is, Do industries self-select themselves in choosing whether or not to apply? By comparing the utility of not applying with the maximum expected utility that can be derived from filing an application, an industry can make a rational decision as to the usefulness of seeking protection from the ITC. By using a nested logit model, one can determine whether or not self-selection occurs.

Figure 1 illustrates the postulated structure of the model for the actors' choices. The model assumes that the regulator's decision is conditional on an industry's choice of applying. Stage 1 is the industry's decision of whether or not to file an application for protection. Stage 2 is the ITC's decision of whether or not to grant protection to the industry. The nested logit model was chosen because it characterizes the two-stage decision process well and allows for dependence among the attributes of the alternatives.

Suppose the utility of final outcome *ri* is represented by U_{ri}. The utility can be rewritten as the sum of the observable components V_{ri} and the unobservable disturbances ϵ_{ri}:

$$U_{ri} = V_{ri} + \epsilon_{ri} \text{ for } i, r = 1, 2,$$

where $i \equiv$ industry choice and $r \equiv$ regulator choice. The strict utility of alternative *ri*, which is defined as the component of utility that is measurable or observable, is denoted by V_{ri} and can be expressed in the form $V_{ri} = W(r \mid i) + W(i)$, where $W(r \mid i)$ is the strict utility from components that depend on both the regulator and the industry decisions, while $W(i)$ is the strict utility from components that depend only on the industry choice. The probability that outcome *ri* is chosen, then, is given by

$$P_{ri} = \text{prob}(U_{ri} > U_{jk} \;\forall\; jk \neq ri).$$

The ϵ_{ri} are the unobserved characteristics and are assumed to have a generalized extreme value (GEV) distribution. I assume a GEV distribution, instead of assuming that the ϵ_{ri}s are independently and identically distributed with an extreme value distribution, because it is likely that including the alternative of industries' not applying with the positive and negative ITC decision alternatives would violate the independence of the irrelevant alternatives assumption of the logit model. The GEV distribution leads to a nested logit structure (McFadden 1978) in which the regulatory commission's decision on granting protection is examined, given that the industry files a petition for regulatory action.

The estimation of the probability of outcome A_2 is done in two stages. First, I estimate the parameters, β, from the conditional probability, $P(A_2 \mid A_2, A_3)$. After defining an inclusive value (INCV) as the log of the denominator of the estimated conditional choice model, I then estimate the parameters, α, from the probability model $P(A_2, A_3)$, given INCV. (See Appendix B for details.)

In order to include the inclusive value as an independent variable in the estimation of P_{A_1}, a value for INCV is needed for all industries—both those that apply and those that do not apply. Using the estimates of the βs from the industries that do apply, an inclusive value is computed for *each* industry. The INCV variable denotes the expected maximum utility that may be obtained by applying for protection, given one's own characteristics; that is, INCV accounts for an industry's anticipation of how the ITC will respond to its application. In estimating the industry choice of whether or not to apply, if the coefficient $(1 - \sigma)$ of the inclusive value is zero (i.e., $\sigma = 1$), industries choose between the alternatives by comparing the utility of *not* applying with the maximum expected utility that they can derive from applying.

Because an inclusive value is needed for all industries, it was necessary to exclude the variables for petitioner type and unionization, which are reported only for industries that actually file a petition.[7] For the nested logit model, then, the matrix of variables that may affect the supply of regulation is defined by X = (elasticity, employment, Ways and Means Democrats, Ways and Means Republicans, trade subcommittee Democrats, trade subcommittee Republicans, dummy variables for the Ways and Means chair and ranking member, trade subcommittee chair and ranking member, capacity utilization, U.S. trade deficit, concentration, percentage change in employment, percentage change in market share, and tariff rate). The matrix of variables that may affect the demand for regulation is defined by y = (concentration, percentage change in employment, percentage change in market share, tariff rate). These supply-and-demand variables are used as the independent variables to explain regulator and industry behavior respectively. The nested logit model, then, is used to estimate the parameters β, α, and σ.

The results of the nested logit model appear in Table 1. On the supply side, the factors that appear best to explain ITC decisions on trade protection are location of the industry in a district whose repre-

Table 1. Coefficient Estimates for the Nested Logit Model

	Determinants of ITC Decisions[a]		Determinants of Industry Decisions[b]	
Variable	Coefficient	t-Statistic	Coefficient	t-Statistic
Constant	-3.68	-2.47*	-2.12	-9.47*
Elasticity of demand	-.31	-.96	—	—
Industry employment	1.14	.67**	—	—
Ways and Means Democrats	-.12	-.98	—	—
Ways and Means Republicans	.20	.64**	—	—
Trade subcommittee Democrats	.61	3.15*	—	—
Trade subcommittee Republicans	-.75	-1.93*	—	—
Ways and Means chair	1.28	2.74*	—	—
Ways and Means ranking member	.09	.14	—	—
Trade subcommittee chair	-.25	-.50	—	—
Trade subcommittee ranking member	-.11	-.19	—	—
Capacity utilization	.95	.62	—	—
U.S. trade deficit	1.38	2.46*	—	—
Industry concentration ratios	-.48	-.05	-3.81	-1.07
Percentage change in industry employment	-2.07	-1.12**	-1.27	-1.82*
Percentage change in market share	7.60	1.74*	-.90	-2.81*
Tariff rate	1.89	1.45	-1.19	-2.58*
Inclusive value	—	—	.18	3.29*
Number of cases	205		2,903	
Percentage correctly predicted	72		92.97	

[a] The dependent variable is the ITC decision: 1 = *protection*, 0 = *no protection*. There were 80 positive decisions and 125 negative decisions by the ITC.

[b] The dependent variable is the industry decision: 1 = *apply*, 0 = *not apply*. There were 205 industry applicants and 2,698 nonapplicants.

*$p \leq .05$, two-tailed test.

**Indicates $p \leq .05$ when the number of congressional representatives for each industry is replaced by a dummy variable.

sentative is a Democrat and a member of the trade subcommittee of Ways and Means, location of the industry in a district whose representative is the chair of Ways and Means, the U.S. trade deficit, size measured by employment, and the industry's percentage change in employment. Because employment is highly correlated with industry representation in the House of Representatives, size and percentage change in employment do not appear significant in Table 1. However, when the congressional representation variables (Ways and Means Democrats, trade subcommittee Democrats, Ways and Means Republicans, trade subcommittee Republicans) are replaced by dummies signifying representation by at least one committee member, size becomes significant at the 2.5% level, and percentage change in employment is significant at the 10% level. (Also, representation on the Ways and Means Committee by Republicans becomes significant at the 5% level when dummies are used here, but the significance of representation by Democrats disappears.) Presidential influence measured by party identification (not in the table) was insignificant with the t-statistic at −0.105.

These results indicate some degree of support for a pressure group model of regulation, especially congressional dominance. The size of the industry measured by the employment variable indicates that larger industries are more likely to get protection than smaller ones. The significance of employment as an explanatory variable lends support to the hypothesis that size matters and that larger industries are better able to secure desired regulatory action, perhaps because of their greater political power relative to smaller industries. Finger, Hall, and Nelson (1982) also find support for the effect of size on ITC injury determinations. (Further, dummy variables for the steel and automobile industries were insignificant and did not affect the overall performance of the model.) In addition to the effect of size, firms are also more likely to get protection if their representatives in the House are on the appropriate committees. Positive coefficients on the variables for congressional representation indicate that industries are more likely to get protection if they are located in districts that have members of Congress on the Ways and Means Committee or trade subcommittee. The results seem to indicate that Congress does have some influence over its agency's decisions.

On a highly visible issue like trade policy, which directly affects a member of Congress's constituents by affecting factors like district unemployment, legislators have to be concerned with voter preferences. This notion is explored by Denzau and Munger (1986), who formally model the supply of public policy in the United States. They conclude that if voters are informed, committee assignments bear a direct resemblance to geographic constituency interests and enable legislators to serve their geographic constituencies (Denzau and Munger 1986, 101–2). Also, Tosini and Tower (1987) found in studying voting behavior on the 1985 textile bill that representatives were influenced by state unemployment levels and the importance of textiles to their states as well as by contributions from textile special interest groups. These studies further support my findings of congressional influence.

As an indicator of economic hardship and need for regulation, the variable measuring percentage change in employment in the industries was included in the analysis. Decreasing employment should be an indicator of a need for protection in an industry, but may also be an impetus for political pressure on policy makers. As indicated above, a percentage decrease in employment means a higher likelihood of protection from the ITC.

Finally, the yearly U.S. trade deficit was used to capture national economic trends that would affect an industry's likelihood of getting protection. This variable, significant at the 1% level, indicates that industries are more likely to get protection, the greater the trade deficit is in the United States.

The hypothesis that industries whose goods have a more inelastic demand are more likely to get protection because of less deadweight loss is not supported. The elasticity variable, a proxy for efficient policies, is insignificant and has the opposite sign from that predicted by Becker's pressure group model. The negative sign on the elasticity coefficient indicates that of those industries that applied for protection in this period, those producing more inelastically demanded goods were *less* likely to get protection than industries producing goods with a more elastic demand. Thus, the notion that regulators make decisions based on the efficiency of their policy choices is not supported by this analysis.

Concentration, which is a proxy for industries' costs of organization, should be a good measure of an industry's ability to pressure for political outcomes. Recall that the regulatory theories discussed earlier implied that the greater an interest group's organization and the lower the costs of that organization, the greater its political power or influence should be But this variable is insignificant in predicting ITC decisions. Though pressure group theory suggests that the more concentrated an interest group is, the better it should be at pressuring policy makers, an alternative hypothesis to consider is that a high degree of concentration in production is not as important in terms of political influence on trade as some other factors, like geographic concentration. Though higher concentration may indicate greater organization in an industry, it may be that more political power is generated when an industry is geographically concentrated in a relatively small number of states or congressional districts than when production is limited to a small number of firms in a number of states. For example, it may be that the lumber industry, which has a very low concentration ratio but is geographically concentrated and especially strong across the Pacific Northwest, will have as much political power in getting trade protection as a highly concentrated one such as the U.S. steel industry. Interestingly enough, when a variable indicating the number of states in which an industry has operations was included in the model, the coefficient was negative and significant at the 2.5% level; that is, the fewer the number of states, the more likely an industry was to get protection. So geographic political strength may be as important as production concentration in terms of political power for industries seeking protection from foreign imports. Another possible explanation for the insignificance of concentration in explaining industry success, which can be drawn from the work of Marvel and Ray (1983), is that concentrated industries are more successful at maintaining their tariff rates by directly pressuring Congress and the president during periods of international negotiations for changes. (Finger, Hall, and Nelson [1982] also argue this point.)

Finally, additional industry-specific indicators of economic need for protection were included in the analysis. These are percentage change in an industry's share of the domestic market due to increased imports, capacity utilization, and existing tariff rates. The larger an industry's loss in domestic market share, the greater the threat posed by foreign imports, and thus the greater its need (or desire) for protection should be. However, the results in Table 1 indicate that the larger an industry's decrease in market share, the less likely the ITC to grant it protection. The implication here is that losing ground to foreign imports does not increase an industry's likelihood of being granted protection by the ITC.

The capacity utilization of an industry, which is a measure of the percentage of plant capacity that is being used in production, was included as an indicator of how well an industry is performing. A low capacity indicates large amounts of idle resources, hence lower production in the industry. The results in Table 1 indicate that industries with a higher capacity are more likely to get protection, but the statistic is insignificant. Though capacity is low in some industries that suffer from foreign competition, it is also the case that industries such as space vehicles and equipment, tanks, small arms and ammunition, and gasoline pumps, which all face relatively little competition from foreign imports, are among the industries with the lowest capacity utilization figures; so capacity appears not to be an accurate measure of an industry's need for protection from foreign competition, and the results confirm this.

Whether or not industries already have high tariffs also does not appear to explain ITC decisions. Though the variable for tariff rates is positively correlated with an applicant's likelihood of being granted protection, it is insignificant. Since a wide range of tariff rates exist in the United States, it seems that the ITC has no sense of an "ideal" rate, implying that existing rates have little effect on ITC decisions.

Percentage change in market share, capacity utilization, and existing tariff rates all appear not to be good indicators of economic hardship and a likelihood of getting protection from the ITC. None of these measures of economic need are significant predictors of ITC decision making, except the change in market share, which has the opposite sign from that expected. (I will show that these factors are significant in explaining demand.) Only national trends, here captured by the trade deficit variable, appear to be a good economic predictor of ITC decisions. Measures of political power or influence appear to perform better in explaining the supply of protection by the ITC. The results show that the influence of particular members of Congress may affect ITC decisions as well as the size of industries, decreases in employment, and the type of petitioner. Clearly, the influence of politics on matters of trade protection cannot be denied.

On the demand side, the results indicate that industries that already have higher tariffs and ones that increase their market shares relative to imports and increase their employment are less likely to apply for protection. Concentration again has the opposite sign from that initially expected, if one believes that concentration implies organization and political power. Industries with lower concentration ratios are more likely to apply than those more highly concentrated. As discussed earlier, concentration in production alone may not be a good measure of political strength and may instead indicate success through other channels, like that of pressuring Congress or the president directly to impose some type of trade barriers.

The coefficient on the variable for percentage change in employment in an industry is negative and significant at the 5% level. This indicates that industries with rising employment are less likely to apply for protection from the ITC. The percentage change in domestic market share shows similar results, with significance at the 1% level. And the variable indicating the present tariff rate, also significant at the 1% level, shows that industries with high existing tariff rates are less likely to apply for more protection than those with lower rates. These results seem to indicate that industries are more likely to apply for protection when they are at an economic disadvantage and are doing poorly.

Also, the issue of labor intensity was addressed by including the ratio of employment over value of shipments in the model. Labor intensity was added as an explanatory variable under Democratic versus Republican leadership in the White House. Neither the supply of, nor the demand for, protection appear to be significantly related to labor intensity, even when the Democrats control the White House. This result supports the findings of Basevi (1966) and Cheh (1976) over earlier results, that labor-intensive industries are not more likely to be protected than capital-intensive ones.

In summary, industries appear to file applications with the ITC when they face economic hardship, but their likelihood of being granted protection, given that they applied, seems to depend less on economic need and more on their ability to apply political pressure and influence the decision makers. Even more striking is the fact that the correlation between congressional power and ITC decisions is significant even when economic hardship and existing levels of protection are taken into account. This lends further support to the notion that signs of congressional dominance may not merely be the result of members' successfully supporting protectionist measures solely on a basis of perceived economic need of applicants in their districts.

However, the most important result from the model is the low coefficient (.18) on the inclusive value variable. This indicates that industries do self-select themselves in deciding to file applications for protection; that is, industries base their decisions to apply or not on their perception of the maximum expected utility of being granted protection by the ITC. The economic and political factors that affect ITC behavior are captured in the inclusive value variable that is used to explain industries' decision of whether or not to apply for protection. Thus, the inclusive value accounts for industries' anticipation of how the ITC will respond to its application. It is this possibility of correlation between the unobserved characteristics that leads to a violation of the assumption of the independence of irrelevant alternatives, which makes the three alternative multinomial logit model inappropriate.[8] The implication here is that an industry acquires some information about the value of its filing an application. The industries that believe their requests will probably be rejected will not bother to apply, since their expected utility from applying will be lower than the utility they derive from not applying. Thus, the applicant pool is *not* a random sample of industries facing economic hardships.

Conclusion

The purpose of this research has been to explain the varying levels of protection across industries by focusing on factors that affect both the supply of, and demand for, the regulation of trade. Both circumstances that lead industries to request protection and factors that affect the government's decision of whether or not to supply it have been examined. Industries and the government presumably have incentives to pursue utility-maximizing courses of action. On the demand side, when an industry seeks a higher tariff, the benefits from that tariff presumably outweigh the costs of applying and lobbying for protection. On the supply side, when the government chooses to protect an industry, the political benefits in terms of, for example, votes or contributions presumably exceed the costs in terms of the lost support from those harmed by the policy. A major problem of the literature on both the ITC and the regulation of trade in general has been a failure of scholars to account for both the supply and the demand sides of the regulatory issue. This is my modest contribution towards filling this void. I test for the potential problem of industries self-selecting themselves when seeking regulation, that is, making their decisions by taking into account what they perceive the ITC's decision will be.

The problem of self-selection is dealt with by modeling ITC outcomes with a *nested structure* (the nested logit model) where the final outcome depends on both industry choice and agency choice. The industries (demanders) face the binary choice of whether or not to undergo the costs of applying and pressuring the regulators for trade protection. The ITC (the supplier) makes the binary decision of whether or not to grant regulatory benefits to each of the applicants based on industry characteristics and political factors. This work demonstrates that self-selection is a problem in predicting the probability of an industry's getting protection; that is, industries do appear to self-select themselves when deciding whether or not to apply for protective tariffs.

Because of this problem of sample selection, much of the scholarly work done on predicting either the demand for, or supply of, protection may have been subject to sample bias. For example, Goldstein (1986) and Takacs (1981) each look at the supply of regulation by the ITC and claim, from their analyses, that protectionist pressures do not affect government policies. What each of these studies fails to account for is that the group of petitioners may have common unobservable characteristics that lead them to apply in the first place. For Goldstein, the fact that the acceptance rate of petitions has not changed over time may merely mean that the characteristics of the pool of applicants has not changed, not that policy makers have been unaffected by protectionist pressures. And, Takacs' claim that the government does not respond to protectionist pressures may be the result of inaccurate predictions about the demand for protection or the factors that drive industries to file petitions. Both models can be improved to predict more accurately the effect of protectionist pressures on policy outcomes. On a more micro level, the results above indicate that pressure groups may indeed affect outcomes. Representation on relevant congressional committees, size (measured by employment), and the type of petitioner (single firms or groups of firms) seem to be particularly good predictors of ITC behavior.

A vast majority of the literature, both on trade regulation in general and on the ITC in particular, focuses only on one side of the problem. Many factors have been identified to explain the supply of, and demand for, protection; but by isolating and examining only one side of the problem at a time, many seemingly false or contestable conclusions have been drawn. A common thread in all of the ITC-specific studies on trade regulation is that the agency is not affected by, and does not respond to, political pressures. This work challenges that result by clearly demonstrating the importance of the problem of self-selection. These findings lead to new avenues for future research into the questions of how interest groups might influence regulators and what effects their pressuring might have on policy outcomes. By looking deeper into the politics of regulation, perhaps we can better understand what drives interest groups to apply for regulation and what determines whether or not they will be granted the regulation they seek.

Appendix A: Description of Independent Variables

1. The *elasticities* of demand are available for 122 three-digit SIC industries (Shiells, Stern, and Deardorff 1985). The elasticities were computed from estimates of cross-price elasticities of U.S. import demand and measures of import share for these industries using Cournot aggregation (Deaton and Muellbauer 1980, 15–17). The elasticities range from −3.86 to 1.94 with a mean of −1.06. (See Appendix C.) Among the industries whose goods are more elastic are the furniture, drapery, and motorcycle industries; while clocks and watches, plastics, and footwear are among the goods with more inelastic demands. Five of the industries in the study have positive elasticity values, implying that higher prices lead to greater demand for the goods in these industries. These are the nonrubber footwear, leather gloves, fur goods, lumber, and radio and television equipment industries. The cross-price elasticities for nonrubber footwear, leather gloves, and fur goods are extreme outliers, so their price elasticity values were left out of the data analysis. It may be the case that the nature of the select market for such goods as fur coats and leather gloves makes demand for these goods increase regardless of a rise in their prices. In other words, higher prices in these markets may not discourage consumption, perhaps because higher prices, in and of themselves, make these luxury items more attractive to their consumers.

2. The *size* of the industry petitioning for protection is measured by the total *employment* at the four-digit SIC industry level (U.S. Bureau of Census 1974–85). From the theories of regulation presented here, size should be a significant factor in affecting policy outcomes. In particular, Becker's theory seems to predict that larger groups will be more successful at influencing the government (given a sufficiently large tax-paying group). Because

of a high degree of correlation between the employment and shipment variables, only one will be used in the analysis. Employment was chosen over value of shipments because there were fewer missing data entries for this variable. Also, the employment variable should perform better as a proxy for size in measuring an industry's influence over policy outcomes because workers face layoffs if domestic protection drops due to increases in foreign imports, and politicians tend to pay attention to the demands of unions or large, organized groups of workers that pressure for legislative action. Employment at the four-digit SIC level in the industries ranges from 600 in the reclaimed rubber industry to over 500 thousand employees in the miscellaneous plastics and radio and television equipment industries, with a mean of 42 thousand overall. Also, the *percentage change in employment* was included to capture trends of decline in the industries.

3. The *petitioners* in the investigations are classified into three categories: single firms (209 cases), multiple firms (317 cases), and the administration (28 cases), which includes petitions filed by members of Congress, a committee of Congress, the president, the special trade representative, and the ITC itself. A variety of industries belong to each type of petitioner category.

4. *Unions* or worker associations are sometimes included among the petitioners. A value of one was assigned to cases that included one of these groups as petitioners, a value of zero to those that did not. These occurred in mostly the blast furnace and steel industries but also included a variety of others.

5. Several variables may be used to capture overall yearly trade-related economic trends in the United States. The variable used in this study is the yearly *U.S. trade deficit.* A higher trade deficit should have a positive effect on an industry's request for protection. This is because policy makers should be more willing to help industries when the overall economy is performing poorly than when it is prospering. The trade deficit variable ranges from a trade surplus of over $9 billion in 1975 to a deficit of over $60 billion in 1983 and $110 billion in 1984.

6. *Capacity utilization* is used as a measure to capture production efficiency in an industry and is also coded at the four-digit SIC industry level (U.S. Bureau of Census 1978, 1980, 1984). The percentage of capacity utilization varies from 100% for certain years in such industries as butter, sugar, cooking oil, periodicals, ceramic tiles, and certain clothing and footwear industries, to 13% in the space vehicle and guided missile industries, with a mean of 69%.

7. An industry's *percentage change in domestic market share* is an important factor which should affect its demand for regulation. If an industry's percentage share of the domestic market drops from one year to the next because of an increase in imports, it should be more likely to seek protection from foreign competition. The change in market share is measured at the four-digit SIC level and is the change in the percentage of the domestic market controlled by the industry. (The data on value of imports, exports, and shipments were made available by Clinton Shiells and Greg Schoepfle, both with the U.S. Department of Labor. See also U.S. Bureau of the Census 1975–85a, 1975–85b.) The industry with the largest percentage increase in market share was the primary metals industry, while the largest decreases occurred in the fabricated rubber industry and in the primary metals industry in a different year.

8. The *existing tariff rates* of industries recorded at the three-digit SIC level should also be an indicator of industry demand for protection. The higher the existing tariff rate, the less likely an industry will be to demand greater protection. These rates are available from the *Tariff Schedules of the United States* for the rele-

vant years. Since no major international trade negotiations were conducted by U.S. presidents in the years covered by this study, the rates, as a whole, did not change significantly over this period of time. Some changes, however, did occur as a result of congressional legislation, limited presidential concessions, or ITC investigations.

9. The *concentration ratio* of the industry should affect both the supply of, and demand for, regulation. Concentration is a measure of the percentage of output by the four largest firms in an industry. The concentration ratio of an industry, recorded at the four-digit SIC level, is used as a proxy for an industry's costs of organization (U.S. Bureau of Census 1967, 1972, 1977, 1982). On the supply side, higher concentration should indicate lower costs of organization, thus a greater ability to produce pressure and affect policy outcomes. On the demand side, a higher concentration ratio means fewer free riders and larger gains from demanding regulation. The primary lead industry had the highest concentration (100%), while specialized industry machinery and certain wood, printing, and concrete industries had concentrations at or below 5%, with the overall industry mean at 39%.

10. By industry and year, the number of Democrats on the Ways and Means Committee in Congress who have the industry located in their districts is given by *Ways and Means Democrats.* This information was obtained by matching industry location by city and county at the four-digit SIC level with congressional districts. Only districts with 250 or more employees in the industry were counted as being represented in any given area. A dummy variable was also used to denote industries that were represented by at least one member of the committee. The next three variables were obtained in an identical fashion: (1) by industry and year, the number of Democrats on the trade subcommittee in Congress who have the industry located in their districts is given by *trade subcommittee Democrats*; (2) by industry and year, the number of Republicans on the Ways and Means Committee in Congress who have the industry located in their districts is given by *Ways and Means Republicans*; and (3) by industry and year, the number of Republicans on the trade subcommittee in Congress who have the industry located in their districts is given by *trade subcommittee Republicans.*

11. By industry and year, dummy variables for presence of *Ways and Means chair, Ways and Means ranking member, trade subcommittee chair,* and *trade subcommittee ranking member* were also used.

12. By industry and year, the number of states in which the industry has operations is given by the variable *states.* This variable was compiled from Bureau of the Census 1977.

Appendix B: Nested Probability Model

Recall that the generalized extreme value (GEV) distribution is defined as

$$F(\epsilon_1, \epsilon_2, \ldots, \epsilon_n) = \exp[-G(e^{-\epsilon_1}, e^{-\epsilon_2}, \ldots, e^{-\epsilon_n})],$$

where the generating function $G = G(Y_1, \ldots, Y_J)$ is nonnegative and homogeneous of degree 1 with $(Y_1, \ldots, Y_J) \geq 0$. Also, $\lim_{Y_c \to +\infty} G(Y_1, \ldots, Y_J) = +\infty$ for $c = 1, \ldots, J$, and for any distinct $(c_1, \ldots, c_k)$ from $\{1, \ldots, J\}$, $\partial^k G / \partial Y_{c_1} \ldots \partial Y_{c_k}$ is nonnegative if k is odd and nonpositive if k is even. It is known (McFadden 1978) that the probabilities derived from the GEV distribution define a choice model that is consistent with random utility maximization. These probabilities are given by

$$P_c = \frac{Y_c(\partial G/\partial Y_c)(Y_1, \ldots, Y_J)}{G(Y_1, \ldots, Y_J)}$$

$$\text{for } c = 1, \ldots, J. \qquad \text{(B-1)}$$

For the case of three alternatives considered in Figure 1 of the text, we can define the function G as follows:

$$G(Y_1, Y_2, Y_3) = Y_1 + [Y_2^{1/(1-\sigma)} + Y_3^{1/(1-\sigma)}]^{1-\sigma}.$$

Then from equation B-1, the following conditional probabilities can be computed:

$$P(A_2|A_1, A_2, A_3) = \frac{Y_2^{1/(1-\sigma)}[Y_2^{1/(1-\sigma)} + Y_3^{1/(1-\sigma)}]^{-\sigma}}{Y_1 + [Y_2^{1/(1-\sigma)} + Y_3^{1/(1-\sigma}]^{1-\sigma}}$$

Conditional on choosing one of the alternatives A_2 and A_3 (e.g., applying for protection and getting a positive ruling or applying and getting a negative ruling), the probabilities have the binomial logit forms

$$P(A_2|A_2, A_3) = \frac{Y_2^{1/(1-\sigma)}}{Y_2^{1/(1-\sigma)} + Y_3^{1/(1-\sigma)}} \qquad \text{(B-2)}$$

and

$$P(A_3|A_2, A_3) = \frac{Y_3^{1/(1-\sigma)}}{Y_2^{1/(1-\sigma)} + Y_3^{1/(1-\sigma)}} \qquad \text{(B-3)}$$

Furthermore, for the probability of an industry applying we have

$$P(A_2, A_3|A_1, A_2, A_3) = \frac{[Y_2^{1/(1-\sigma)} + Y_3^{1/(1-\sigma)}]^{1-\sigma}}{Y_1 + [Y_2^{1/(1-\sigma)} + Y_3^{1/(1-\sigma)}]^{1-\sigma}} \qquad \text{(B-4)}$$

Assuming that $U_{ri} = V_{ri} + \epsilon_{ri}$ where $V_{ri} = \log Y_{ri}$ denotes the strict utility of alternative ri, we can now identify the components of the strict utility, V_{ri}. Recall that V_{ri} is the utility of the final outcome A_1, A_2, or A_3. $W(i)$ is the component of the utility that depends only on the industry choice, namely W (apply) or W (not apply), while $W(r\,|\,i)$ is just W (positive | apply) or W (negative | apply). As $V_{ri} = W(r\,|\,i) + W(i)$, we assume that W (not apply) $= -\alpha' y$; W (apply) $= 0$; W (positive | apply) $= (1 - \sigma)\beta' X$; and W (negative | apply) $= 0$.[9] Then by substitution,

$$V_{A_1} = W\,(\text{not apply}) = \log Y_1 = -\alpha' y$$

$$V_{A_2} = W\,(\text{positive}\mid\text{apply}) + W\,(\text{apply}) = \log Y_2 = (1 - \sigma)\beta' X$$

$$V_{A_3} = W\,(\text{negative}\mid\text{apply}) + W\,(\text{apply}) = \log Y_3 = 0,$$

where the outcomes A_1, A_2, and A_3 are depicted in Figure 1 of the text. The vector X comprises the observed characteristics that are believed to explain the regulator's choice of whether or not to grant industry i protection (the supply of regulation), and y is the vector of observed characteristics that vary with an industry's choice of whether or not to apply (the demand for regulation). Next, by substitution, it is possible to calculate the probabilities of the alternatives at the two different stages of the decision tree. (Details of the computations of the probabilities are available on request.)

Using equations B-2 and B-3, the probabilities for the ITC decisions on whether or not to grant protection, given that an industry applies, may be written as

$$P(A_2|A_2, A_3) = \frac{1}{1 + e^{-\beta' X}} \qquad \text{(B-5)}$$

$$P(A_3|A_2, A_3) = \frac{e^{-\beta' X}}{1 + e^{-\beta' X}}$$

For the industry decision, using equation B-4, the probabilities of applying and not applying have the following forms:

$$P(A_2, A_3 | A_1, A_2, A_3) = \frac{1}{1 + e^{-[\alpha' y + (1 - \sigma)\mathrm{INCV}]}} \qquad \text{(B-6)}$$

$$P(A_1 | A_1, A_2, A_3) = \frac{e^{-[\alpha' y + (1 - \sigma)\mathrm{INCV}]}}{1 + e^{-[\alpha' y + (1 - \sigma)\mathrm{INCV}]}}$$

where the inclusive value, INCV $\equiv \log(1 + e^{\beta' X})$.

Finally, from equations B-5 and B-6, the probability of an industry's getting protection, given that it applies, may be written as

$$P_{A_2} = P_{A_2|\{A_2,A_3\}} \bullet P_{\{A_2,A_3\}|\{A_1,A_2,A_3\}} = \frac{1}{1 + e^{-\beta' X}} \bullet \frac{1}{1 + e^{-[\alpha' y + (1 - \sigma)\mathrm{INCV}]}}$$

A sufficient condition for a nested logit model to be consistent with stochastic utility maximization is that the coefficient of the inclusive value, $(1 - \sigma)$ lies between 0 and 1 (see, e.g., McFadden 1978). When the coefficient of the inclusive value is 1, we get the multinomial logit form for P_{A_2}, where the three alternatives are independent. When the coefficient is 0, the probability of an industry's getting protection is the product of two logit elements, that is,

$$P_{A_2} = \begin{cases} \dfrac{e^{\beta' X}}{1 + e^{\beta' X} + e^{-\alpha' y}} & \text{if } (1 - \sigma) = 1 \\[2ex] \dfrac{1}{1 + e^{-\beta' X}} \bullet \dfrac{1}{1 + e^{-\alpha' y}} & \text{if } (1 - \sigma) = 0. \end{cases}$$

The estimation of the probability of outcome A_2 may be done in two stages. First, we estimate the parameters, β, from the conditional probability $P(A_2 | A_2, A_3)$. After defining an inclusive value, INCV, as the log of the denominator of the estimated conditional choice model, we then estimate the parameters of α from the probability model $P(A_2, A_3)$, given INCV.

Appendix C: Descriptive Statistics

Table C-1. Descriptive Statistics of the Variables

	Statistics for All Industries (2,903 Cases)				Statistics for All Applicants (205 Cases)			
Variable[a]	Mean	Minimum	Maximum	Standard Deviation	Mean	Minimum	Maximum	Standard Deviation
ELAS	-1.11	-3.86	.08	.45	-1.18	-2.88	.08	.52
CONS	40.79	4.00	100.00	21.03	39.33	7.00	98.00	22.67
CAP	70.37	20.00	100.00	12.87	68.97	29.00	97.00	11.68
EMP	45.49	1.30	528.30	66.18	87.19	1.30	505.40	121.50
PRCHGEMP	-.007	-.47	7.74	.18	-.023	-.31	.47	.09
PRCHGMKT	-.008	-4.39	8.36	.22	-.012	-1.54	.24	.12
TARIFF	36.05	0	114.5	16.51	33.36	6.70	66.41	14.10
DEMWM	1.64	0	15.00	2.19	3.54	0	15.00	2.93
DEMTR	.69	0	8.00	1.18	1.93	0	8.00	1.69
REPWM	.59	0	7.00	1.08	1.11	0	5.00	1.40
REPTR	.35	0	5.00	.76	.72	0	4.00	1.07
STATES	20.49	1.00	50	10.32	24.0	3.00	46.00	11.60

[a] The variable names for the abbreviations are as follows: ELAS = elasticity, CONS = concentration, CAP = capacity utilization, EMP = employment, TRDEF = trade deficit, TARIFF = tariff rate, PRCHGEMP = percentage change in employment, PRCHGMKT = percentage change in domestic market share, DEMWM = industry representation by Democrats on the Ways and Means Committee, DEMTR = industry representation by Democrats on the trade subcommittee, REPWM = industry representation by Republicans on the Ways and Means Committee, REPTR = industry representation by Republicans on the trade subcommittee, STATES = number of states in which an industry is located.

Notes

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1. See U.S. International Trade Commission 1975-85 for excerpts from the original texts and a description of these six statutes.

2. The applicants must provide fairly substantial amounts of information to the agency on the merchandise, foreign manufacturers, prices of domestic and foreign producers, transportation costs, insurance charges, third-country involvement, evidence on less than fair value or subsidization, volume and value of foreign merchandise, and the like. Also, filing a petition has costs in terms of the signal that is sent to the ITC. Firms are not likely to file petitions when the case is truly bogus for fear of damaging their future possibilities of success; that is, they do not want to cry wolf too often.

3. The value of shipments by industry was also considered as a proxy for size, but employment was chosen as the more appropriate measure because of its political overtones.

4. Cassing, McKeown, and Ochs 1986 explore the idea of geographical regions' being more or less protectionist based on whether the industries in the region are expanding in investment and production capacity or contracting. The changing economic stakes of different regions generate a tariff cycle, and interest groups are organized along these geographic cleavages.

5. Since the dumping and subsidization decisions were taken away from the Treasury Department and giving to the Commerce Department in January 1980, the potential problem of pooling the data from the earlier years with the later years of this study is also tested. A dummy variable is included in the model for cases after 1979. The results indicate that this change was not a significant factor in explaining the ITC's decisions.

6. It has been suggested (see, e.g., Finger, Hall, and Nelson 1982) that the types of cases that the ITC

investigates are different in nature and hence subject to different political and economic criteria. Both Wald and log-likelihood ratio tests were used to test the hypothesis that a restricted model treating antidumping, countervailing duty, and escape clause cases differently by including dummy variables for each type of case performs better than the unrestricted model to be presented here. However, this hypothesis is rejected at the 1%-level of significance for both tests.

7. Looking purely at the supply of protection, the type of petitioner did appear to be a good predictor of ITC behavior. The results indicate that petitions filed by groups of firms were more likely to get positive ITC rulings than those filed by single firms. However, the effect of union involvement, though positive, was insignificant.

8. A detailed account of the argument can be obtained upon request.

9. Note that the utilities are normalized such that W (apply) = 0 and W (negative | apply) = 0.

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OPPORTUNITY, WILLINGNESS, AND THE DIFFUSION OF WAR

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Using borders and alliances as indicators of opportunity and willingness, respectively, we test the relationship between these and the diffusion of war during the 1816–1965 period. The impact of borders and alliances, individually and in combination, on the growth of ongoing war through "infectious" diffusion is shown through the comparison of baseline cases to cases where states at peace were exposed to various "treatments" comprised of warring border nations or warring alliance partners. The findings indicate that the probability of war diffusion is substantially increased as opportunities and willingness increase, particularly when such geographic and political factors are combined. The applicability of the opportunity and willingness framework to the study of war and diffusion is expanded and confirmed.

A considerable amount of the early empirical research on war attempted to explain its onset by looking at the effects of one or several independent variables on a dependent indicator variable, such as, for example, the nation-months of war (Singer, Bremer, and Stucky 1972; Singer and Small 1968) or the number of nations at war (Singer and Small 1974). The initially unrecognized problem with this procedure is the conflation of the onset of war (a dichotomous variable) with the size of a war (a continuous variable). The problem, however, has consequences significantly beyond what type of measurement is appropriate, since by using the *size* of a war while the theory under investigation specifies that *onset* is being measured, the distinct possibility of diffusion is overlooked. This means that the process by which the first two nations in a war *begin* fighting may be considerably different than the process by which subsequent participants *join* the war. This blurred distinction neglects what is usually referred to as Galton's problem (Ross and Homer 1976). Under circumstances where diffusion is present but unrecognized, the resulting models are necessarily misspecified and most probably investigated with inappropriate methods.

Because of the recognition of this problem, more recent research has moved in two new directions. One of these focuses on the behavior of the initial participants in wars and attempts to explain only their behavior. To a considerable extent this is accomplished by considering the onset of war as the end product of a dispute between nations (Bueno de Mesquita 1981; Bueno de Mesquita and Lalman 1986; Leng and Gochman 1982; Maoz 1982).

A second line of research is based on the recognition that wars might diffuse, or be "contagious."[1] In general, research on the diffusion of war began with determining the extent to which wars were "infectious" (Davis, Duncan, and Siverson 1978; Faber, Houweling, and Siccama 1984; Most and Starr 1981) and, after establishing that they were, seeking to uncover the factors responsible for *variation* in the diffusion processes. In general, two lines of investigation have been followed on the diffusion of war. The first of these is based upon borders as interaction opportunities (Most and Starr 1980; O'Loughlin 1984; Ward and Kirby 1987). The second centers on alliances as *indicators* of groups of states that share roughly the same international policies and may be willing to fight together for them (Altfeld and Bueno de Mesquita 1979; Siverson and King 1980).

These two lines of research on the diffusion of war—borders and alliances—have only recently been connected in preliminary empirical analysis (Most et al. 1987; Siverson and Starr 1988). In the present research the data set has been expanded significantly over previous collections, new analytic tools have been added and greater attention is given to interaction of method, theory, and empirical findings. In particular, the research we present brings borders and alliances together within a theoretical framework based on the ideas of opportunity and willingness. The use of this framework permits us to examine ongoing wars as events that alter the incentive-constraint structures perceived by foreign policy decision makers, thus increasing the chances that nations will become involved in an ongoing war. The analyses presented here will be conceived in terms of a research design in which particular conditions or sets of conditions are "treatments," which may or may not produce effects.[2]

Opportunity and Willingness in the Diffusion of War

Following the work of Most and Starr, the diffusion analyses presented here will be developed within the general framework provided by the concepts of opportunity and willingness (Most and Starr 1989, chap. 2; Starr 1978a). As ordering concepts, they form the basis for linking environmental and systemic factors to the behavior of decision makers and governments that represent states.

By *opportunity* we mean the possibilities that are available to any entity within any environment, representing the total set of environmental constraints and possibilities. While opportunity thus represents macro level (environmental and structural) factors, willingness represents the choice processes that occur on the micro level, that is, the selection of some behavioral option from a range of alternatives. This framework is derived from Sprout and Sprout's (1969) "ecological triad" of the relationship among entity, environment, and entity-environment. In it their concepts of environmental possibilism, environmental probabilism, and cognitive behaviorism require the combination of both structure-environment and choice-decision process, captured in opportunity and willingness, respectively. Thus, opportunity and willingness are concerned with the relationships that nest decision makers within their surrounding environments.

The central use of *opportunity* is as the degree of interaction. This conception of opportunity has been the primary impetus in Most and Starr's work on the effects of borders as interaction opportunities in the diffusion of violent conflict. As in Sprout and Sprout's environmental possibilism, this simply means that some activity must at base be physically, technologically or intellectually possible. Once the obstacle of possibility is crossed, however, opportunity is, in fact, a continuous phenomenon in which some nations have more or less of it with respect to other nations.

The dual nature of opportunity-possibility (especially as it relates to capabilities) must be recognized. Initially, some capability (technology, ideology or religion, form of government, manner of organizing people to some task, etc.) must be created so as to be part of the range of possibilities available to at least some members of the international system. However, there is then an important second dimension in the effects of opportunity-possibility—the distribution of such capabilities in the international system. At one level all international actors share the same menu of possibilities—for example, no nation could have had nuclear weapons to fight World War I; the United Nations could not have been used to separate the United States and Mexican forces in 1846; and any nation may avail itself of the International Court of Justice. At another level international actors may have very different menus of possibilities—for example, the wealth, technological talent, and resources needed to take advantage of the nuclear possibility are not evenly distributed across nations today.

The concept of *willingness* is more familiar, being central to the study of decision making and choice. The dynamics of choice are embedded in a decision maker's image of the world, or definition of the situation. Willingness is related to a decision maker's calculations of advantage and disadvantage, cost and benefit, considered on both conscious and unconscious levels. It is through willingness that decision makers recognize opportunities and then translate those opportunities into alternatives that are weighed in some manner.

As is implied above, opportunity and willingness are linked in a number of ways. They do not create mutually exclusive categories. Anything that affects the structural possibilities of the environment or environments within which decision makers must act also affects the incentive structures for those decision makers. Opportunity and willingness thus become more than organizing concepts. They take on theoretical characteristics when we understand that they describe the conditions that are necessary for the occurrence of events. They are necessary but not sufficient for the occurrence of international outcomes such as war.

Opportunity, conceived of as interaction opportunity, has been central to the geopolitical study of positive spatial diffusion. This has been most explicitly and extensively developed by Most and Starr, who use borders as the interaction opportunity through which violent conflict would spread. Borders are investigated and conceptualized as "constraints on the interaction opportunities of nations" (Starr and Most 1976). Simple geographic proximity, as indicated by borders of various kinds, is related to both the amount of interaction and the probability of various types of interaction, such as war (see Starr and Most 1978). To some extent, borders represent a proximity between states that increases both the salience of the neighboring territories and (in general) the ease of interaction. Thus, because states are probably more likely to be more attuned to, and involved in, political situations on their borders, there is a tendency for opportunity to shape the range of willingness—although power certainly affords some nations the possibility of very wide openings for defining their borders. Briefly, the various works of Most and Starr argue that opportunity for interaction is a necessary condition for the positive spatial diffusion of violent conflict, that borders are one factor providing such an opportunity, and that the empirical evidence for the 1946–65 period indicates that positive spatial diffusion of violent conflict occurred along the easements provided by borders.

It needs to be emphasized that within such a warring border nation framework it is not reasoned that borders *cause* wars but rather that they contribute to the potential outbreak of violence because the more borders a nation has, the greater (1) the number of risks and opportunities confronting the nation, (2) the likelihood that the nation or its territories will be "conditionally viable" (Boulding 1962), and (3) the level of that nation's uncertainty. Under these conditions, it is asserted, nations have a greater probability of going to war (see also Diehl and Goertz 1988).

Additionally, Most and Starr argue that once a war starts on a nation's border, that nation may find its environment changed in such a way that it either participates in a war it did not intend to join or forgoes fighting a war that it had intended to join. The warring border nation model thus analyzes whether states that experience the treatment of having a warring nation on their borders have their environment and decision calculus altered so as to alter the probability of their own subsequent war involvement, or not. (See Starr and Most 1985 for a full elaboration of the warring border nation model.)

While borders can in general be conceptualized as an "agent" of diffusion representing the effects of opportunity, another such agent, alliances, represents willingness.[3] Numerous observers of alliances in international relations have commented on their entangling nature. Following this general line of argument, a second approach to the problem of war diffusion has thus focused on the extent to which alliance commitments could be responsible for the diffusion of wars.

Siverson and King (1979, 1980) explore the extent to which the independent effect of alliance memberships and the attributes of different types of alliances account for the extent and character of war diffusion. Recognizing the work of Most and Starr on diffusion and borders, Siverson and King argue that alliances, unlike geography, result from a deliberate process of policy choice. States clearly have much greater latitude in their choice of allies than the states on their borders.[4] It is thus important to recognize alliances as *manipulable* interaction opportunities.

More importantly, it is reasonable to look at alliances as a conscious choice among foreign policy behaviors or policy positions. The willingness to form alliances—and with specific partners—may be seen as an indicator of shared policy preference. Put simply, two (or *n*) nations forming an alliance are indicating, to some degree, that they share policy preferences.[5] This conception of alliances, implicit in the work of Siverson and King, is explicitly developed in Bueno de Mesquita's (1981) model of expected utility, an approach centered on the calculations that lead to willingness.[6]

The research we report here seeks to investigate the effects of both opportunity and willingness on the diffusion of war. This will be done by looking at the individual and combined effects of borders and alliances on the diffusion of war among members of the international system over the period 1816–1965.

Research Design and Data Generation

While several studies of conflict diffusion have relied on varieties of autocorrelation (Hill and Rothchild 1986; O'Loughlin 1984), the method we chose is considerably simpler. It has much in common with various epidemiological models in that it examines the magnitude of the effects of being exposed to various conditions as treatments. More specifically, it allows us to explore various changes in the probability that nations will enter an ongoing war if they have a warring border nation (WBN) or a warring alliance partner (WAP).

In order to evaluate the impact of WBNs and WAPs on the diffusion of war we constructed a data set containing several types of information on national borders, national alliance commitments, and national war participation. Data were collected and coded in order to test both a WBN hypothesis and a WAP hypothesis. Thus, the WBN hypothesis and the WAP hypothesis may be tested separately, and the strength of the results compared. The data also permit us to look at the impact of a nation's bordering states that are *also* allies. This will enable us to test the combined treatment of a nation's being both a WBN and a WAP. Again, the results of these analyses may be compared to the results of the analyses of each factor singly.

In order to make the appropriate comparisons, however, it is necessary to do more than simply record the relevant border and alliance information for the instances in which nations entered a war. Doing so would tell us something about the process of diffusion, but such a procedure would deal only with "successful" cases in which diffusion took place. The cases in which a nation experienced some type of either a WBN or WAP but did not enter the war would be lost from view; hence, no estimate could be made of the effect of the variable on the larger population within which these nations exist (see Most and Starr 1989, esp. chaps. 3–5).

In order to pursue our purpose of assessing the joint and individual effects of borders and alliances on the war experiences of states, it was necessary to bring together data on war participation, alliances, and borders of each state in the international system for each year between 1815 and 1965. Information on the set of states in the system, war participation, and national alliance commitments was relatively easy to acquire. The set of states was taken from the lists provided in Small and Singer (1982). We also noted the power status of the various nations, using a simple division of nations into either major power or minor power status.[7]

There are, of course, several generally available data sets on international war, including, most notably, those of Kende (1971, 1978), Richardson (1960), Small and Singer (1982), and Wright (1965). Most and Starr's initial diffusion analyses used a combination of all these sources, as those analyses, in actuality, investigated the diffusion of any organized violent conflict (whether it was civil war, internal conflict, or intervention or relatively small-scale violence) as well as large-scale organized interstate violence. We, however, focus on interstate war for the entire state system over the post-Napoleonic era.

We thus selected the larger-scale interstate war data set presented in Small and Singer 1982, which represents an updating and refinement of a well-established earlier effort by the same authors (Singer and Small 1972) to present the data of the Correlates of War Project. There were several other reasons to select this war data set. First, it incorporates a great deal of the information contained in the earlier studies by Wright (1965) and Richardson (1960). The data used here cover more contemporary events than the compilations by Wright and Richardson but also cover much earlier periods than the post-1945 data of others (e.g., Kende). Finally, utilizing Small and Singer's data will make our findings compatible with the growing body of empirical work that has derived from the Correlates of War Project (Gochman and Sabrosky 1990).

The alliance data also are a product of the Correlates of War Project. In this case we drew upon Sabrosky's (1975) extensive revision of an earlier work by Singer and Small (1968) that provided an initial listing of formal international alliances between states during the period 1815–1965. In the case of each alliance, we identified its class as coded by Singer and Small: defense, neutrality, or entente. These will be referred to as A1, A2, and A3, respectively.

We view these types of alliance commitments as forming an ordinal index of willingness. Using Sabrosky's discussion, Garnham (1988, 15) outlines these three types of alliances as developed by the Correlates of War Project: "The strongest alliance commitment is a defense pact, . . . in which the signatories agree to intervene militarily in the event of an attack on one of their number. Next, insofar as the formal strength of the alliance is concerned, is the neutrality or nonaggression pact, . . . which obligates the signatories to remain militarily neutral should one of them become involved in a war. . . . Finally, the entente . . . merely required consultations or conversations if one of the signatories was attacked." Thus, we expect that WAPs involving defensive alliances (A1) will have a greater likelihood of producing diffusion than nonaggression pacts (A2) or finally the ententes (A3).

The most difficult data to gather were those indicating which states shared borders. However, through the use of several excellent historical atlases (Shepherd 1932 and *Hammond Historical Atlas of the World*), it was possible to ascertain the border network of states back to 1815. Specifically, in a modified version of the coding rules used by Starr and Most (1976), we recorded for each state the entities on its contiguous borders, those across less than two hundred miles of open water and those on the borders of its colonial possessions.[8] We refer to these as B1, B2, and B3, respectively.

As noted, we regard the divisions of the border and alliance variables as having an ordinal value. In terms of the opportunity and willingness concepts discussed we hypothesize that the greatest opportunity is present with contiguous borders, then cross-water borders, and finally, colonial borders (Starr and Most 1976). At that point considerable variation is introduced. Similarly, the greatest willingness should be present with defense alliances, then neutrality agreements, and then ententes.

Using the nation-year as the basic unit of analysis, the completion of the data collection eventuated in a data set of 3,929 cases, representing a coding of the borders, alliances, and war participation of all nations in all years between 1816 and 1965 in which a war either started or was ongoing. However, to make our test as rigorous as possible we removed a number of cases. First, in order to test for diffusion as the growth of war it was necessary to remove the initial two participants in a war. Second, once a nation entered a war it was deleted from the data set until the war was concluded or the nation left the war. These reductions produced a final data set of 3,749 cases. Within this data set there were 94 cases of war diffusion whose existence we will attempt to explain on the basis of opportunity and willingness.[9]

In order to differentiate positive spatial diffusion from both reinforcement phenomena and negative spatial diffusion, a set of expectations was derived and tested. The procedure used here is derived from one used in an earlier study by Starr and Most (1983, pp. 110–11).[10] In their design they began by "looking for all states at any given point in time which were at peace (to avoid complications with 'reinforcement' effects) and asked two questions: (1) at what point in time did they have any warring border nations? (2) Within the next five years did they have any new war participations?"

There is one major deviation in the present study from the design set out above. Instead of looking at a treatment at time $t0$ and subsequent behavior only during the $t1$–$t5$ period, we have looked at all years ($t0$) in which the international system was experiencing war (some 83 years between 1816 and 1965) and examined the extent to which nations not at war in $t - 1$, either were or were not exposed to various combinations of WBNs or WAPs and either did or did not go to war in year $t0$. Recall that once a nation enters a war, it is removed from the data set until the war is ended or the nation leaves the war. Under this procedure it is possible then to aggregate the results of all war years for each of the treatments.

This procedure made it possible for us to estimate and compare the effects of various combinations of WBNs and WAPs. Moreover, by running identical tables for the conditions of any border or any alliance we were able to estimate the extent to which WBNs and WAPs made a difference over counterpart conditions where war as a condition or treatment was absent; that is, we could compare the effects of having a contiguous WBN to the effects of simply having contiguous border nations or compare the effects of having a defense pact WAP to the effects of simply having defense pact partners. Delineating the effects of the various types and combinations of borders and alliances *without* WBNs or WAPs thus gives us a set of baselines for comparing the war-joining rate of those states *with* WBNs or WAPs.

The logic of our investigation of diffusion centers on the notion of treatment, that is, the notion that the environment of the decision makers of states will have been changed by the existence of war in a bordering nation or in one of its alliance partners. As developed by Starr and Most in regard to borders (1976), and in terms of foreign policy decisions in general (Most and Starr 1989), such treatments may alter the opportunities facing decision makers as well as their willingness to pursue certain behavioral alternatives, such as going to war. The argument also follows our notions that states' interactions will tend to follow along the paths provided by interaction opportunities such as proximity (borders) and common policy interests–high value salience (alliances).

The theoretical basis of opportunity and willingness, as well as for interaction opportunity, has been developed at length by Most and Starr (1980, 1989). The WBN model derives from the notion of interaction opportunity—the physical possibilities for interaction, factors that increase the probability that such interaction will take place, and the perception of both possibility and probability by decision makers. This idea is rooted in the ecological and geopolitical concepts and formulations of Sprout and Sprout (1969), as later modified by Starr's concepts of opportunity and willingness. It should be recognized that the concept of opportunity encompasses important components of the geographical theory of proximity and its effects (Boulding 1962; Gleditsch 1969; and Zipf 1949). For example, Boulding's loss-of-strength gradient links geographical components to threat perception and the role of uncertainty (Midlarsky 1975).

Our expectations on the effects of such treatments also may be seen within a "loose necessity" framework; that is, these treatments are to be seen as loosely necessary but not sufficient for influencing the war-joining behavior of states. We are not arguing that such treatments always lead to joining ongoing wars but that war behavior is much more likely to occur if such treatments have occurred. This is the "loose" aspect of the logically necessary relationship between treatment and consequence. The key to these expectations is cell d in Table 1, which sets out our basic treatment matrix. Cell d indicates that a state has joined a war subsequent to being exposed to the relevant treatment. The central expectation is that

Table 1. The Border-Alliance Treatment Matrix

War Participation	Treatment	
	Absent	Present
Absent	a	b
Present	c	d

there should be a clear and strong difference in cell d between matrices where there is a treatment and matrices where there is *no* treatment; that is, analyses presented will contrast the results found in a matrix where the columns simply note the presence or absence of *borders* or *alliances* to the results of a matrix that looks like the one presented in Table 1, where *warring* border nations or *warring* alliance partners comprise the treatment columns.

This is a simple idea: being exposed to a treatment will, using Alcock's (1972) phrase, increase the chances of "catching the war disease." We ask what the world should "look like" if a state has a bordering state at war or an alliance partner at war. Our expectation is that a WBN or WAP should substantially increase the probability of that state's joining the ongoing war of its WBN or WAP. A further expectation is that this probability should increase with the combination of border and alliance treatments. Thus, we have taken one of several possible approaches to diffusion—the growth of ongoing wars. We have identified two possible ("loosely necessary") agents by which those wars would grow. We have then posited a simple expectation of behavior given 'treatments' by those agents. The following section presents data that simply but directly test these expectations.

Findings

Initially, it may be noted that the overall rate of war diffusion is rather small. In the 3,746 nation-years in our data set there were only 94 cases of war diffusion. The overall rate of diffusion is thus 2.51%. War diffusion is, to be sure, a relatively rare event, but *rare* does not mean "unimportant." Many statistically rare events are of considerable interest to scientists, particularly when their consequences are either highly lethal or very costly. For example, both lung cancer and earthquakes are both relatively rare, but are nonetheless the objects of attention for very large, nationally organized research efforts. War diffusion is both lethal and costly. Of the 240 cases of international war involvement in the Singer and Small data set, the 94 cases we studied represent 39% of the total. Additionally, a great deal of the severity of war (i.e., how many die) may be explained in terms of the expansion or growth of war. In fact, from the data in Small and Singer's (1982) consideration of lethality in warfare, it is clear that most of the war-related deaths take place because of war expansion.

Because war diffusion is a relatively rare event, it may be seen that under "loose necessity" a large number of treatments result in a much smaller number of cases of war diffusion. In this respect several points should be noted. First, we investigate here only one form of diffusion—infection, or the growth of an ongoing war. We do not deal with demonstration effects. Also if only the *opportunity* for interaction was of concern, we might expect ongoing wars to grow to include all those nations with opportunity. However, willingness is important in such decisions. Put differently, decision makers choose behavior within the constraints posed by the range of incentive structures within which they are imbedded. The effects of WBNs and WAPs are just one aspect of that structure. While other aspects of the structure may lead to the willingness to choose other foreign policy behaviors, it is impressive that the opportunity and willingness model of interaction opportunity is able to identify WBNs and WAPs as factors having a significant impact on war-joining behavior.

The data analysis will take place in two stages. First, we will explore the extent to which the variables measuring opportunity and willingness have anything to do with war expansion. Put differently, we will show that there is a significant relationship between the various measures of

Table 2. No Treatment, Treatments, and War Involvement, 1816–1965

War Involvement	Border or Alliance (No Treatment) No	Border or Alliance (No Treatment) Yes	Border or Alliance (No Treatment) Total	Warring Border or Warring Alliance Partner (Treatment) No	Warring Border or Warring Alliance Partner (Treatment) Yes	Warring Border or Warring Alliance Partner (Treatment) Total
No	14	3,641	3,655	2,320	1,335	3,655
Yes	1	93	94	8	86	94

opportunity and willingness and proclivities to join a war. That done, the second part of our data analysis will be to examine how various individual and combined treatments affect war-joining behavior.

Do Opportunity and Willingness Influence War Diffusion?

From the previous discussion of opportunity and willingness it is obvious that we expect nations that have been exposed to either WBNs or WAPs will have a higher propensity to join a war than those that have not. We begin our analysis by examining the extent to which this expectation is borne out. Table 2 reports two initial parts of this analysis. The left half presents the cross tabulation of a nation's having any border or any alliance and being a war joiner. Except for the 15 cases in the first column, this baseline is the same as the overall baseline. It is readily apparent that the table contains no relationship.[11] Moving to the right half, it may be seen that when a nation is exposed to any of the treatments, the propensity to join a war increases substantially. Put simply, in the baseline case 2.4% of the nations participate, but under the treatments 6.1% join.

While the left half of the table demonstrates that exposure to any treatment increases the likelihood of joining a war, we need to examine the effect of increasing numbers of treatments. Nations may at the same time have various types of warring borders, may be the members of several alliances of different types having members in the war or, more likely, have some combination of warring borders and alliances. Table 3 displays the cross tabulation of war involvement against the number of treatments to which a nation was exposed. It is clear from the data that as the amount of exposure increases, the rate of participation increases as well. The overall relationship (see Table 4) as measured by the correlation ratio (eta), derived from a one-way analysis of variance, is .452.

The analysis has not thus far clarified which of the variables makes the greatest difference. There are, unfortunately, some difficulties in making such an assessment. Our dependent variable, war in-

Table 3. War Diffusion and Number of Treatments to Which a Nation Was Exposed

War Involvement	0	1	2	3	4	5	6	> 6
No	2,320	703	372	142	57	34	12	15
Yes	8	9	17	12	10	16	7	15
Percentage yes	.3	1.2	4.3	7.8	14.9	32.0	36.8	50.0

Table 4. Analysis of Variance for Treatments in Table 3

Source of Variation	Sum of Squares	*df*	Mean Squares
Treatments	18.737	13[a]	1.441
Error	72.906	3,735	.20

Note: $F = 73.839$, $p < .001$, eta = .452.

[a] The degrees of freedom are from the uncollapsed table.

volvement, is dichotomous, which means that the usual techniques of regression and correlation are not appropriate. Normally, one could approach this type of a problem with log-linear analysis, but an analysis based upon dichotomies of the six possible independent variables and war involvement would produce a table of 128 cells. Given the large total number of observations we have, this would not ordinarily be a problem; but in this instance only 94 of the cases indicate war diffusion, and allocating these in a very large table would produce such a large number of empty cells that analysis would be questionable.

Fortunately, it is possible to gain estimates of the effects of the various independent variables on war diffusion through probit, which, although sensitive to the highly skewed distribution of the data will allow us to make a preliminary comparison of the relative effect of the variables on war diffusion. Table 5 displays the results of the probit analysis when a nation's war involvement is analyzed in relation to its number of (1) contiguous WBNs (B1); (2) cross-water WBNs (B2); (3) colonial WBNs (B3); (4) defense WAPs (A1); (5) neutrality WAPs (A2); and (6) entente WAPs (A3). The estimates it gives for the independent variables are generally equivalent to the estimates of beta reported in regressions. The results given in Table 5 are generally consistent with the ideas of opportunity and willingness; that is, for the border variables B1 has greater weight in influencing war joining than B2, and B2 has more than B3. The alliance variables are slightly different. While A1 counts the most among these variables, A3 influences war joining much more than A2; in fact, from this analysis A2 seems to have little or no effect on war joining.[12]

Table 5. Probit Estimates of Treatment Variables on War Involvement, 1816–1965

Parameter	Estimate	*t*-Statistic
Constant	-2.62	-31.80
B1	.54*	9.97*
B2	.36**	3.47**
B3	.21*	3.77*
A1	.51*	5.68*
A2	.18****	1.36****
A3	.31***	3.14***

Note: B1 = contiguous borders, B2 = cross-water borders, B3 = colonial borders, A1 = defense alliances, A2 = neutrality agreements, and A3 = ententes.

$^{*}p < .01$.
$^{**}p < .02$.
$^{***}p > .05$.
$^{****}p > .10$.

How Much Do the Variables Contribute to War Diffusion?

Thus far, our analysis has indicated that any exposure to the treatment variables substantially increases the chances of a nation's joining a war (Table 2), that more exposure increases such chances (Table 3), and that the strength of the impact of the treatment variables is ordered approximately as our rationale for opportunity and willingness suggested (Table 5). What the analysis has not revealed is the magnitude of the impact of the individual and combined indicators of opportunity and willingness.

Let us begin with a brief, but necessary, description of the relatively straightforward analysis that follows. Recall from Table 2 that we compared the rate of war joining among all nations to only those nations that had been exposed to at least one treatment variable. We may now, in effect, take Table 1 apart, examine the impact of each of the variables individually on war diffusion, and then combine each of the border variables with each of the alliance variables to observe their combined impacts on war diffusion.

The first 3 rows of Table 6 report the results, for all nations of the baseline, of being exposed to one of the three types of WBNs or WAPs and the percentage change associated with the treatment.[13] Beginning with borders, two things are apparent. First, the presence of a WBN has a significant impact on war diffusion. In each case the presence of the treatment produces a marked increase in the propensity for diffusion, ranging from a 262% increase for the contiguous borders to slightly over 100% for the colonial borders.[14] Second, the strength of the results is ordered as the opportunity hypothesis predicts, with the closest borders producing the strongest result and the most distant borders producing the weakest result.[15]

Turning to the alliance variables shown in Table 6, a similar pattern may be seen, but with considerably stronger results. Defense alliances produce the substantial increase of 439% over the baseline. While the results for neutrality agreements and ententes are weaker, 297% and 166%, respectively, they are still strong. Again, the strength of the relationships is ordered from the strongest to the weakest commitments. As agents of the opportunity for interaction, both borders and alliances meet the expectation of increasing the probability of states' joining ongoing wars. In addition to the increased ease of interaction that borders provide (opportunity), borders and alliances both individually increase the salience of the WAPs and WBNs and thus also the willingness to become involved in their conflicts.

How do the border and alliance variables interact with each other? The three border and three alliance variables combine, of course, to produce nine possibilities. The first three columns of Table 7, report the results of these combinations for all nations. (Because the tables reporting the combination of the variables contain eight cells, the cell corresponding to d in Table 1 is h.) It may readily be seen that there is a considerable amount of interaction between the border and alliance variables in influencing the diffusion of war. The most potent effect is with the combination of the contiguous WBNs and defensive WAPs. This should not be surprising, given our previous results and our hierarchy of importance within opportunity and willingness. The latter derives clearly from the international relations literature. Most geopolitical studies focus on direct contiguous borders because of their immediate impact and because they provide the most important opportunity for interaction. In turn, the form of alliance that, scholars theorize, creates the strongest bond between states is the defensive pact. Defense pacts provide the most important indicator of salience, commitment, and shared policy preference. The magnitude of the joint effect is, to say the least, considerable, with the combination of these two variables producing an increase of 719% over the baseline cases in producing war diffusion.

While that result is the strongest, the other results on the several combined variables also generally reveal a considerable amount of interaction. Indeed, *all* contiguous border (B1) and all noncolony

Table 6. Effects of Individual Border and Alliance Treatments, 1816–1965

	All Nations			Major Powers			Minor Powers		
	Cell d Percentage			Cell d percentage			Cell d percentage		
Condition	No Treatment	Treatment	Percentage Change	No Treatment	Treatment	Percentage Change	No Treatment	Treatment	Percentage Change
B1	2.4 (3,300)	8.7 (772)	262*	7.2 (304)	17.5 (103)	143*	1.9 (2,996)	7.3 (669)	284*
B2	3.1 (1,696)	9.2 (295)	196*	7.7 (353)	24.4 (78)	216*	1.9 (1,343)	3.7 (217)	94
B3	2.6 (1,992)	5.6 (659)	115*	9.4 (256)	17.9 (123)	90*	1.6 (1,736)	2.8 (536)	75**
A1	5.3 (808)	28.6 (105)	439*	9.9 (191)	40.0 (40)	304*	3.9 (617)	21.5 (65)	451*
A2	4.4 (204)	17.5 (40)	297*	5.9 (67)	27.3 (11)	362††	3.6 (138)	13.8 (29)	283**
A3	4.2 (453)	11.2 (160)	166*	10.2 (137)	26.5 (49)	159*	1.6 (316)	4.5 (111)	181*

Note: Numbers of cases are in parentheses. B1 = contiguous borders, B2 = cross-water borders, B3 = colonial borders, A1 = defense alliances, A2 = neutrality agreements, and A3 = ententes.

*$p < .01$ (difference of proportions).
**$p < .05$ (difference of proportions).
†$p < .01$ (Fisher's exact test).
††$p < .05$ (Fisher's exact test).

Table 7. Effects of Combined Border and Alliance Treatments, 1816–1965

	All Nations			Major Powers			Minor Powers		
	Cell h Percentage			Cell h percentage			Cell h percentage		
Condition	No Treatment	Treatment	Percentage Change	No Treatment	Treatment	Percentage Change	No Treatment	Treatment	Percentage Change
B1A1	5.1 (755)	41.8 (55)	719*	9.2 (152)	37.0 (27)	302*	3.9 (603)	46.4 (28)	1,089*
B1A2	4.4 (204)	22.2 (27)	404*	5.9 (67)	40.0 (5)	577††	3.6 (137)	18.2 (22)	405*
B1A3	3.6 (211)	18.5 (65)	413*	9.5 (105)	30.8 (26)	224	1.6 (306)	10.2 (39)	537*
B2A1	6.8 (355)	47.8 (23)	602*	10.6 (161)	61.1 (18)	476†	3.6 (194)	0 (5)	-100
B2A2	4.5 (112)	30.0 (10)	566††	6.7 (60)	75.0 (4)	1,019†	1.9 (52)	0 (6)	-100
B2A3	6.4 (250)	33.3 (27)	420*	10.9 (128)	64.3 (14)	489†	1.6 (122)	0 (13)	-100
B3A1	6.5 (352)	34.1 (44)	424*	15.1 (106)	70.0 (20)	363†	2.8 (246)	4.2 (24)	50
B3A2	4.8 (83)	14.2 (14)	195	7.3 (41)	66.7 (3)	813††	2.4 (42)	0 (11)	-100
B3A3	5.5 (253)	15.3 (72)	178*	11.8 (102)	40.0 (25)	238*	1.3 (151)	2.1 (47)	61

Note: Number of cases are in parentheses. B1 = contiguous borders, B2 = cross-water borders, B3 = colonial borders, A1 = defense alliances, A2 = neutrality agreements, and A3 = ententes.

*p < .01 (difference of proportions).
***p* < .05 (difference of proportions).
†*p* < .01 (Fisher's exact test).
††*p* < .05 (Fisher's exact test).

cross-water (B2) effects are improved when combined with alliance variables; and all defense and neutrality pact (A1 and A2) effects are improved when combined with B1 and B2. Cross-water WBNs and defense WAPs, for example, combine to produce an increase of 602% over the baseline cases, while cross-water WBNs and neutrality WAPs combine to increase war diffusion 566% over the baseline cases. Even colonial borders (B3) and ententes (A3), the weakest of the individual effects, produce significant increases on war joining when combined with the other variables—indeed, even when combined with each other (178%).

Given the various combinations of our variables it is, of course, difficult to produce an ordered prediction of their strength, as we did with the single variables. However, it is worth noting that when the class of the WBN is held constant and the WAP class varies, with one exception the magnitudes of the impact are ordered within the groups. The sole exception is that the combination of contiguous WBNs is stronger with entente WAPs (413%) than it is with neutrality WAPs (404%).

Thus far, our consideration of the problem of war diffusion has treated all nations as if they were the same except for their exposure to the various treatments. Much of international relations theory, however, relies on the knowledge that all nations are not the same. A distinction among nations that traditionally has been central to an overwhelming part of the field of international relations is that between major and minor powers. For our purposes, too, this is an important distinction, since there is ample reason to believe that major and minor powers will differ in their behavior with respect to opportunity and willingness in joining wars. Specifically, we should expect the major powers to respond to opportunities that are cross-water or colonial because, in the first instance, they have the greater capabilities necessary to become involved and, in the second instance, they not only have the capabilities but are much more likely to have colonial borders. Moreover, while minor powers certainly had many alliances, major powers are overrepresented among the nations with alliances, particularly defense agreements.

The neatness of the analysis begins to break down when we analyze the data separately for the major and minor powers in the international system. We begin with the major powers. The effects of the individual border and alliance conditions for the major powers are given in the middle three columns of Table 6. Here it is evident that while each of the variables had a discernable effect on war diffusion, the results depart from the magnitudes and orderings given in the first three columns of the same table. Cross-water WBNs are the strongest border effect for the major powers as are neutrality WAPs. As we expect from our ideas about the strength of various forms of opportunity, colonial WBNs have only a weak effect.

Note, however, that alliances are generally of more importance for major powers than are borders and that this relationship is stronger for major powers than for all states combined. By definition, major powers are the actors with global (or at least multiregional) interests and capabilities. They would be more likely to be able to dominate smaller neighbors. As such, the greater impact of alliances is not surprising.

The middle three columns of Table 7 present the combinations of variables for the major powers. Again, there is clear interaction involved in these combinations, some of them astonishingly large. Indeed, *every* individual condition improves with the combined effects of borders and alliances. The combination of colonial WBNs and neutrality WAPs, for example, produces an increase over the baseline of 813%. As might be suspected in the case of such a sharp increase, the number of cases is small ($N = 3$). Similarly, the cross-water WBNs and neutrality WAPs produce a gain of 1,019% for four cases.

More surprising, perhaps, than the size of some of the gains observed is the fact that the combination that previously demonstrated the largest gain, the combination of contiguous WBN and defense WAP, for the major powers, shows only a modest increase. Here the gain is only 302%; and while such an increase is not to be ignored, its modest size relative to some of the other combinations of variables is not in accord with what we would expect from our interpretation of opportunity and willingness or the findings presented above. The significant increase found in the effects of B3 (colonial borders) in combination with all alliance types may be related to Most and Starr's (1980) findings on colonial borders above. While the numbers of cases are rather small, it is clear that the combined effects of colonial borders *and* alliances—(far-flung) opportunity and willingness—now match the earlier findings and arguments about the interests and capabilities of major powers. These findings give us some anticipation of what will be found with the minor powers, to which we now turn.

The last three columns of Table 6 report the individual effects of WBNs and WAPs for the minor powers. First, it may be noted that with two glaring exceptions (B2 and B3, where the increases are very low) the treatment variables had clear effects. The strongest of these was the presence of defense WAPs, where the increase was 451%. In addition, the results are ordered as our discussion of the strength of the various indicators suggests. Comparing the results for major powers (the middle three columns) and minor powers, the latter are more strongly affected *both* by contiguous borders (B1) and defense pacts (A1). The first finding is expected. Minor powers, virtually by definition, are those with local or regional concerns. Given the limited ability of minor powers to project power and thus their greater concern with viability in terms of immediate neighbors, direct borders should be of greatest importance.

The A1 results, however, are more intriguing. Previous work by Siverson and King (1979) has demonstrated that alliances are likely "agents" of diffusion. Such research results directly address the war-alliance relationship that is of concern to many scholars and the related assertion that alliances act as *conduits* for the spread of international conflict (e.g., see Starr 1978b). Alliances might bring great power conflict into the regional subsystems of small allies. Conversely, alliances might drag major power allies into the local conflicts of their smaller alliance partners. Since the bulk of minor power defensive alliances are with major powers, the A1 results appear to indicate in which direction the alliance conflict conduit has tended to work—minor powers being pulled into the ongoing wars of their major power allies.

The last three columns of Table 7 report the results of the nine combinations of variables for the minor powers. Again, some of the increases are, to say the least, sharp. The situation where we expect the largest increase, contiguous WBNs and defense WAPs, does indeed show the strongest increase, with a magnitude of over 1,000%. Moreover, this time, the number of cases is *not* small. The first four variable combinations all show marked increases, then something interesting takes place.

Once the more distant forms of opportunity and willingness are encountered, the results drop substantially. In fact, for the last six combinations in the table the results are either meager or nonexistent. For all of these cases, the combined treatment results are *less* than the effects of the individual treatments; that is, the combined conditions make war diffusion less likely. For the first time the results show negative interaction. One may initially suspect that this result is nothing more than the artifact of variable combinations producing numbers of cases so small that they disappear. But, as shown in the last three columns of Table 7, this is not the case. While the number of cases does go down because of the combination, they are certainly too big simply to attribute the absence of an effect to that artifact.

The more likely explanation is to be found in the fact that these combinations do indeed represent the more distant forms of opportunity and lesser types of willingness and that minor powers, because of their lesser capabilities, are either reluctant or unable to enter conflict. Wars, after all, do impose costs on participants; and minor powers may not occupy positions where the costs are tolerable.

A Problem?

Before drawing the results together there is a problem, or puzzle, that must be addressed. Throughout the data analysis it may be seen that there is a consistent pattern wherein the number of nations exposed to the various treatments is very large relative to the number of nations who actually join the war. Consider, for example, the data in the right half of Table 2, which offer the clearest display of this pattern. The data in that table show 1,421 cases of nations exposed to any of the treatments; but only 86 instances of the exposure result in the nations joining a war. To be sure, the proportion of those joining under the treatment is considerably larger than under the baseline, but it is nonetheless perplexing that only 6.44% of the cases exposed to any form of the treatment joined a war.

There are a number of possible approaches to evaluating the impact of these "error" cases to the validity of our findings. First, recall that we explicitly stated that we are working within a framework of "loose necessity." Under this circumstance we expect that there will be many "error" cases but that such error is more apparent than real. The requirements of opportunity and willingness do not predict that a nation exposed to these conditions will join a war but only that its probability of joining will be substantially higher than where those conditions are not present. This situation is quite similar to that encountered by Bueno de Mesquita (1981) in his consideration of the initiation of war. In his analysis it may be seen that nations have positive expected utility for war far more than they actually go to war, while in this research there are far more WBN-WAP interaction opportunities than there are instances of diffusion.

In this research the importance of the idea of necessity may be seen in Table 1. The concern with overprediction is reflected in the number of cases in cell b. However, for necessary relationships the number of cases in b is irrelevant. The relevant cell to compare to cell d is c, which should be empty. Given our "loose necessity" formulation, we should expect only a few cases in c, especially as a percentage of the total number of cases in the no-treatment column (i.e., the a + c column). Returning to the right half of Table 2, it is clear that although there are a very large number of cases in the no-treatment column (2,320), there are only eight in cell c. Similar relationships occur in the matrices that provide most of our results. (In the example given in n. 13 reporting the results of contiguous WBNs there are only 27 cases in c out of a possible 2,977 cases.)

There is a second way to investigate the "error" cases. It is instructive to examine once again the data in the right half of Table 2. There it may be seen that most of the "error" is to be found in the cases in which a nation was exposed to only one of the treatments. The 703 "error" cases

Table 8. Occurrence of Single Border and Alliance Treatments, All Nations, 1816–1965

Variable	Number of Single Treatments	Percentage of Total
B1	328	46
B2	75	10.5
B3	259	36.4
A1	10	1.4
A2	6	.8
A3	34	4.8

Note: B1 = contiguous borders, B2 = cross-water borders, B3 = colonial borders, A1 = defense alliances, A2 = neutrality agreements, and A3 = ententes.

under that condition represent 52.7% of the total "error." As exposure goes up, the rate of war joining goes up quite sharply. The question now is whether or not the 712 cases (i.e., the 703 cases of "error" and the 9 cases of war joining) that received only single treatments are distributed evenly across the opportunity and willingness variables. Table 8 summarizes this distribution. Clearly, exposure to a single B1 or B3 happened fairly frequently but did not have much of an effect.

There is at least one interesting implication that may be drawn from the above observations: decisions to join an ongoing war face considerable "friction." A single opportunity is rarely associated with war joining. It is only when the opportunities begin to accumulate, or, more importantly, are attached to the *political* affinity indicated by an alliance that the chances of joining an ongoing war begin to build significantly. The fact that wars are, in a sense, undersubscribed is not surprising. Theories of collective goods tell us that it is not rational for a nation to undertake a costly course of action if someone else will do it for them (Olson and Zeckhauser 1966) and wars can be among the most costly of all the courses of action a nation may choose.

Conclusion

This study began with the aim of investigating the diffusion of war—in terms of the expansion of ongoing conflict—between 1816 and 1965. This was done in the context of opportunity and willingness, as indicated by WBNs and WAPs. We have proposed a relatively simple expectation derived from the opportunity, willingness, and interaction opportunity concepts and investigated it through a set of comparisons between treatment and no-treatment groups of data. The data presented in the tables support the contention that war does expand among nations along the indicators that operationalize opportunity and willingness and that the strength of the relationship is ordered by the type of border or alliance, as hypothesized.

We have added to the understanding of war diffusion in several ways. While one body of previous research has indicated an important geographic basis for diffusion and other research has indicated that alliances are important in spreading conflict, the present work has the virtue of *combining* geographic and political variables so that the strong interaction of these two elements may be observed.

The scope of the present analysis is also more extensive than previous studies of war diffusion. The data used are global (not limited to any single geographic region) and cover all international wars from 1816 to 1965.[16] We provide separate analyses of major and minor powers, revealing different patterns of diffusion and indicating how major and minor powers differentially relate to the elements of opportunity and willingness.

Perhaps most useful to the theoretical development of the study of war diffusion, we have presented more evidence as to the applicability of research based on opportunity, willingness, and their interaction. We have shown here that this theoretical approach is applicable to large-scale war as delineated by the Correlates of War Project's data set. The previous applications of this approach by Most and Starr focused violent conflict on a smaller scale, whether internal or external.

We also have more confidence in the theoretical scope of this approach, since we now find that it can help explain two different conceptualizations of diffusion: (1) the linkage-penetration view taken in earlier research by Most and Starr (among others), where events elsewhere change a state's disposition to behave similarly; and (2) the infection-contagion approach taken here, where diffusion is conceived as the growth of ongoing wars.

The refinement of such analytic tools is vital in the design of future research. With multiple sources of evidence indicating that diffusion processes of various kinds do exist in international relations, we now require further work in the specification of the *processes* at work in various kinds of diffusion, including the conditions under which the diffusion of violent conflict takes place. The further specification of these processes might also help us understand the interaction between opportunity and willingness and various theories as to why wars are likely to be larger at particular times than at others.

Notes

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1. While *diffusion* has been given several meanings, and has a number of dimensions, we will be using it essentially to mean the growth of an ongoing conflict, the process by which states join an ongoing war and the scope of the conflict becomes enlarged. The concept of diffusion and its theoretical relevance to the study of international conflict is reviewed extensively in Most, Starr, and Siverson 1989. For other reviews of the diffusion concept, see Most and Starr 1981, O'Loughlin 1984 and Welsh 1984. For an overview of the renewed interest in geopolitical approaches (including diffusion) to war, see Diehl 1988. For a classic statement on the contagion of political conflict, see Schattschneider 1960.

2. We use the term *treatment* to emphasize the epidemiological nature of our research, not to suggest that we are performing an experiment in which we have the ability to randomize and control exposure. However, what we report is in many respects a historical experiment, similar in method to the procedures described by Singer (1974) and Holsti and North (1965).

3. Although research on this point is not clear (Garnham 1988), it is reasonable to suspect that under some circumstances geographical proximity to, or shared borders with, some other nation will in themselves influence a nation's willingness to become involved in conflict. If so, opportunity and willingness are not independent, as we noted. Indeed, borders themselves can be seen in terms of environmental possibilism by providing possibilities for interaction. The warring border nation model, however, can be seen in terms of cognitive behaviorism or willingness, as activities on a state's borders effect the perceptions of threat, uncertainty, and opportunity held by that state's decision makers. Clearly, most nations are likely to be sensitive to the security concerns of neighboring nations or, when the capabilities are present, are likely to see their neighbors as the greatest potential threats to their own security.

4. Of course, states may attempt to manipulate their immediate political geography by creating neighbors (e.g., Belgium in 1830) or eliminating them (e.g., Poland in 1939), but the costs of these activities are likely to be quite high.

5. Alliances, of course, are not a perfect indicator of willingness. For example, while there is general tendency for alliances to be reliable, it is clear that reliability is less than complete (Sabrosky 1980). In addition, some alliances are specific to certain issues or geographic areas (e.g., NATO), and their existence does not mean that one ally will join another no matter where the conflict. Also, alliances signed at one time may deteriorate while continuing to exist formally. In short, we do not construe alliances as indicating a general willingness to fight. However, alliances have long been recognized as the key means that nations have chosen to indicate their political position in the international system. Formal alliances are matters of serious concern to decision makers and in addition to their presumed rewards of enhanced security, they impose costs and risks both domestically and internationally (Sullivan 1974).

6. In fact, Altfeld and Bueno de Mesquita (1979) successfully apply expected utility theory to the problem of explaining which side nations will join in a war. While this is a problem similar to the one pursued here, it does not directly address the question of diffusion. For some other differences, see n. 16.

7. Following Small and Singer (1982), the major powers and the years of their inclusion in that group are Austria-Hungary (1816-1918), Italy (1861-1943), the United Kingdom (1816-1965), Russia-USSR (1816-1965), Japan (1895-1945), Prussia-Germany (1816-1945), the United States (1989-1965), France (1816-1940, 1945-65) and China (1949-65).

8. This third category of data was the most difficult to obtain because of the number and almost unbelievable obscurity of some colonies. We were aided very considerably by Henige's (1970) comprehensive list of colonial governors. This list, which begins with the fifteenth century, identifies all political units that were the colonial possession of some other nation. Finding them in this list then permitted relatively easy location in one of the historical atlases.

9. Copies of the coding scheme employed in this study are available from Siverson. It should be noted that in general we followed the data given by Small and Singer (1982) with respect to war expansion. We did, however, depart from their delineation of what constituted war joining in a few cases. For example, they show Japan as joining World War II on 7 December 1941. While it is true that Japan began fighting the United States on that day, it is also true that the Japanese decision had relatively less to do with the European war that had been in progress since 1939 than it did Japan's war with China that had been ongoing since 1937. Hence, we do not treat Japan as joining the war. Also, we do not treat as war joiners the nations that left World War II (albeit briefly) and then changed sides (i.e., Italy, Romania, and Bulgaria) to reenter it. It makes sense to count the initial participation only. It should be noted that including these cases would have favored the opportunity and willingness hypotheses.

10. Recall that Most and Starr (1980) distinguished between reinforcement, or addiction (where a state's war behavior affects the probability of its *own* subsequent behavior) and diffusion (where a state's war behavior affects the probability of the subsequent war behavior of other states). They also recognized that either could have positive or negative effects (either increasing or decreasing the probability of war behavior). The reader must be alerted that the matrix employed in the present analyses follows that utilized in Most et al. 1987 and *is not the same* as the matrix used in Most and Starr 1981 or Starr and Most 1983, 110-11.

11. The single case of war involvement by a nation with no bordering nations of any kind and no alliance involvement is New Zealand's entry into World War II in 1939.

12. The weak results from A2 may be partly due to the fact that there were far fewer of this type of alliance in the data set than either of the other two types. When they are analyzed together, the more numerous alliances may overwhelm A2. In the analysis that follows it may be seen that A2 does not apply to a large number of cases but that when it does, a clear effect is evident.

13. Recall the treatment matrix from Table 1. In the single treatment tables that follow, the numbers being reported are those from cell d of that figure. Reporting all the tables would be cumbersome, so only the effects of the treatments are given. As an example of what a table looks like, we offer a full table for the *treatment* involving B1. Note that the numbers of cases reported in the tables refer to the total of the b-d column and that the percentages reported are for d of that total.

	Contiguous Warring Bordering Nation	
War Participation	Absent	Present
Absent	2,950	705
Present	27	67
Total	2,977	772

14. What we mean by percentage increase is the magnitude by which involvement in a treatment situation is larger than the base. Thus, the first three columns of Table 6, while the B1 treatment percentage of 8.7 is more than three times the size of the baseline percentage of 2.4, the relevant measurement is the amount of increase from 2.4 to 8.7. Thus, the appropriate calculation is the treatment percentage minus the base percentage with the remainder divided by the base percentage.

15. The tests of significance used in Tables 6 and 7 require a brief comment. Because the percentage changes between the no-treatment and treatment columns are often based on relatively small numbers of cases, the skeptical reader may be curious as to (1) whether the observed changes are statistically significant and (2) whether there is any way that the method could fail to produce a change, i.e., whether the results are predetermined. With respect to the first question, we have tested the differences for statistical significance in two ways. First, when the number of cases in the treatment column is greater than 20, the p-value is derived from the Z-score in a test of a difference of proportions. When the number of cases is less than 20, we have determined the p-value from Fisher's exact test. (Recall that the cases in the treatment column are also present in the baseline no-treatment column. Because the two results are therefore not independent, before we could test for statistical significance between the no-treatment baseline and the treatment, it was necessary to remove the treatment cases from their respective baseline. A full set of the resulting tables is available from Siverson.) More important than each test of significance is the correspondence between our theoretical structure and the overall pattern of the

data. With respect to the second question (whether the results are predetermined), if one suspects that the method will always produce an increase in the observed cases of war diffusion under the treatment condition, see the last three columns of Table 7, where failure does occur.

16. Altfeld and Bueno de Mesquita (1979) are interested only in those cases of war expansion that took place within the first two months of a war. Hence, their main data set contains only 40 cases of war joining. In addition, they exclude from their data set the participation of the four Commonwealth nations (i.e., Canada, Australia, New Zealand and South Africa) that joined World War II in 1939, on the grounds of missing data (that is, they had no alliance memberships). Those cases are included here. We also include the cases from the Korean War, which were totally excluded by Altfeld and Bueno de Mesquita (p. 94, n. 7).

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LEGITIMACY, RELIGION, AND NATIONALISM IN THE MIDDLE EAST

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The significance of legitimacy to regime maintenance has been much neglected in recent investigations of the Third World, particularly by behavioralists and rational choice theorists. I define legitimacy, discuss factors that may have contributed to this neglect, and explore the significance of nationalism and religion as major sources of legitimacy in the Middle East. Both a misunderstanding of the role of higher values and rationality in individuals' relationship to social systems and a faulty projection applied to the mainsprings of behavior in other cultures have distorted the perceptions of a number of Western analysts. The relationship between religion and nationalism is complex. Contrary to the common assumption in the West, Islam in general has generated fairly sophisticated constitutional theories. Islamic fundamentalism in particular has been a major source of innovation and adaptation—as well as of spiritual gratification—for the Muslim masses.

Western and Marxist analysts of the politics of the Third World have been primarily concerned with economic development, the organized instruments of physical force, bureaucracies, class structures, and foreign policy. While these are no doubt significant and their investigation has substantially contributed to our understanding of the political phenomenon, they are not sufficient for explaining the level of political stability because they do not provide adequate insight into why people are willing to sacrifice their lives and their material possessions for or against a regime. Even such propositions as inequality or lack of liberties do not seem adequate by themselves. Many systems have had a great deal of poverty, inequality, and repression for long periods without any revolution (Brinton 1965, 32–33; Tocqueville 1955, 176–77). In fact, Brinton (1965, 65–66), Durkheim ([1951] 1966, 246), Huntington (1968, 265–73) and Johnson (1982, 182) consider a lessening of distance between classes and rapid economic development—because they create a crisis of the public (old) order—as sources of instability that contribute to revolutions. An interesting hypothesis by Davies (1971) proposes that the optimum condition for social unrest exists when a sustained period of improvement in material conditions is followed by a sudden and sharp reversal. He refers to the role of expectations in the minds of the people but does not relate such expectations to legitimacy. Obviously, sharp reversals do not always result in a revolution, as a comparison of the political consequences of the depression of the 1930s in the United States and those of the considerably less severe economic conditions of the last years of the Shah's regime in Iran clearly demonstrates.

I propose that the significant missing in-

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gredient in most studies of the politics of the Third World has been an adequate appreciation of the role of legitimacy, primarily a psychological-moral phenomenon. The presentation will proceed through two steps. The first will offer a definition of legitimacy and analyze factors that may have contributed to its deemphasis or neglect, particularly among the behavioral and rational choice analysts; and the second will investigate religion and nationalism in the Middle East in order to demonstrate the significance of legitimacy for regime maintenance.

Values and Legitimacy: The Incongruities of Individual and Collective Rationality

The term *legitimacy* has been used in two different but related ways. The first refers to a set of norms and values relating to politics that are sufficiently shared to make a political system possible. These norms constitute a major component of what is sometime referred to as "the moral basis of authority" (Dekmejian 1985a), a "political culture" (Pye and Verba 1965; Wildavsky 1987), or "a legitimacy style" (Binder 1962) and are acquired mainly through the mechanisms of socialization. They deal with such matters as the primary purposes of the government; the rights and obligations of the government and the governed; and the methods of selection, change, and accountability of the governing personnel. The first task of research is to find out the content and sources of these values in every political system, particularly since "institutionalization"—considered essential to political stability—(e.g., Huntington 1968), is not possible in the absence of shared values.[1]

The second meaning, more appropriately corresponding to the common usage of *legitimacy,* "refers to the extent to which the relevant portion of the population *perceives* that the regime is behaving" according to, or violating, the above norms (Razi 1987, 461–62). As Rustow notes in the case of charismatic legitimation, "a leader's charisma is in the minds of the followers" (1970a, 16). It is thus the people's "positive or negative judgment of what is perceived" about the behavior of the regime that must be investigated to know the level of legitimacy (Eckstein and Gurr 1975, 197).[2] Furthermore, most of the norm clusters that have formed the bases for various crises of the Third World (e.g., Binder et al. 1971) are parts of legitimacy values, although their disaggregation for the purpose of analysis is an entirely reasonable intellectual enterprise. In other words, when the articulate members of a population are by and large satisfied with the government's actions in the areas of identity, participation, distribution, equality, and sovereignty according to the norms they believe in (not necessarily those of the Western or Marxist regimes), there is no crisis of legitimacy.

Consequently, the significant issue is not so much the existence of inequalities, restraints on individual freedoms, and the use of force (from which no political system can escape completely) or even the level of political participation (which varies considerably among stable as well as unstable political systems) as it is whether the degree to which these features exist is considered just or unjust in terms of the shared values. The propositions of relative deprivation (particularly the more promising variants of Galtung 1964 and Gurr 1970), inadequate participation (e.g., Green 1982, 1984; Nie and Verba 1975), and inequality (e.g., Jabbari 1981; Muller and Seligson 1987) as causes of instability and revolution thus become more relevant and meaningful when placed within this framework. As Brinton noted in 1938, "Revolutions cannot do without the word 'justice' and the sentiment it arouses" (1965, 35, 65–66); and Deutsch stated, "People feel that government is just or unjust, legitimate or illegitimate, not only by how it came to power, but also—and mainly—by what it does. If its actions and omissions violate their basic values, they may conclude, as Saint Augustine did in *The City of God*, that 'a government without justice is a great robbery' " (1980, 15). Rustow has developed the idea further and related it to regime maintenance by proposing the formula political stability = legitimacy of institutions + personal legitimacy of rulers (1967, 157).

Why is it, then, that in spite of its significance to stability—particularly in the Third World (Bendix 1978; Binder 1962, 1971; Hudson 1977; McLellan 1983; Pye 1971)—legitimacy has received so little attention (see el-Fathaly and Palmer 1980 for an exception) in the behavioral and rational choice studies of the politics of that region? Pye, who aptly notes the particular suitability of behavioral studies for "illuminating the mystique of legitimacy and the dynamics of authority," considers their emphasis on microlevel analysis as part of the explanation (1971, 144). The following eight factors also seem relevant.

First, a serious problem has emerged from inadequate appreciation of the significance of higher values to resolving a universal problem for the collectivities, namely, the incongruities between the rational search for individual or subunit interests on the one hand and ensuring the interests and indeed the survival of the collectivity on the other. Clearly, the higher values are not necessarily rational or functional at the individual level. It is, for example, rational for an individual not to vote at all and to cheat in every instance he or she can. But if every one becomes "rational" in this individual sense, the system will collapse. As Russett and Starr note, Yossarian of Heller's *Catch-22* is perfectly rational when he refuses to fly bombing missions in World War II and responds to the suggestion that his country might lost as a result, 'We won't lose. We've got more men, more money and more material. There are ten million men in uniform getting killed and a lot more are making money and having fun. Let somebody else get killed.' Major Major replies, 'But suppose everybody on our side felt that way.' Yossarian's answer is devastatingly to the point: 'Then I'd certainly be a damn fool to feel any other way. Wouldn't I?' " (Russett and Starr 1985, 507; Heller 1961, 107). In other words, if everyone takes care of his or her own interest, the interest of all will not be necessarily taken care of. Adam Smith's natural identity of interest is a chimera, particularly in politics.[3] This problem is usually referred to by the economically oriented political scientists—who have little concern with either psychology or the role of morality—as the "free rider" dilemma in securing "collective goods." It has been fully illustrated by the "tragedy of the commons" (Hardin 1968; Pirages and Ehrlich 1974) as well as Rousseau's "stag hunt" and the prisoner's dilemma (Jervis 1978; Snyder 1971). The solution to this tragedy is at least partly provided by norms and metanorms (Axelrod 1986). Consequently, more emphasis needs to be placed on the functions of such norms and mechanisms and the processes by which they are internalized and in most cases voluntarily obeyed.[4]

Second, there has been an insufficient grasp of the difference between the nature and sources of legitimacy and those of performance (i.e., the production of goods and services and generation of organized instruments of physical compulsion). Performance factors and their agents—bureaucracies and technology—can obviously be evaluated and improved in efficiency by universal rational standards (within the limits of our existing level of knowledge), provided there is an agreement on goals or at least on the ultimate values justifying the chain of ends-means relationships of which the given goal is a part. In contrast, the norms of legitimacy "are *arational* and the product of history, tradition and ideologies. They are sociologically primordial, psychologically superego related, morally sacred (in contrast to secular interest calculations), and operationally inculcated through political socialization (citizenship training)" (Razi 1987, 463). People do not as a rule give their lives—let alone their material possessions—for a few dollars more or (as the realists would have us believe) for establishing their selfish power over yet another. Perhaps some individuals do so, and *they* project that the rest of the world is as they are. Without life, however, such matters would have little significance to even minimally "rational" individuals. Whether a U.S. soldier, a Japanese kamikaze, or an Iranian youth clearing the minefields by committing martyrdom—a person as a rule sacrifices life voluntarily for cherished beliefs and values. In other words, "rationality itself must rest on a ground of values which it can neither produce nor discover on its own" (Binder 1971, 40). A misunderstanding of this phenomenon has led to an underemphasis on the role of beliefs and values not only in the conceptualizations of the behaviorally oriented and "rational choice" students of comparative politics and international relations in the West but also among political scientists of Marxist persuasion. It has also caused such analysts as Habermas to try to rationalize the bases of legitimacy on the ground that "every effective belief in legitimacy is assumed to have an immanent relation to the truth . . . that can be tested and criticized" (1968, 292). In Foucault's Nietzschean orientation, the misunderstanding results in nihilism. He argues that "for modern thought, no morality is possible. . . . Sade, Nietzsche, Artaud, and Bataille have understood this on behalf of all who tried to ignore it" (1970, 328). The problem is that without morality, there is no society; and without the latter, even Sade and Foucault would not be able to survive and perform as they aspire to do.

Third, the previous problems have been exacerbated by the lack of explicit distinction in the usage of the terms *interest* and *value*, particularly since Lasswell 1959. Rather, they have been used interchangeably to refer to material interests and the search for power as well as those sacred and superego norms whose function is to regulate the gratification of material interests and the behavior of the governing personnel.

Fourth, the previous problems have in turn interacted with the lack of sufficient attention to the interdependence of performance and legitimacy. It is clear that the more legitimate a government, the more likely its decisions will be implemented voluntarily. Conversely, lack of legitimacy implies resistance and obstructionism on the part of the people and, if present on a large scale, is likely to result in massive challenges to the very survival of a regime in order to remedy perceived injustices. Failure in performance is also likely to affect the legitimacy of the leaders as the difficulties of the regime of Ayatollah Rouhollah Khomeini in the areas of economics, technology and foreign policy indicate. Success in performance areas, however, does not necessarily result in an increase in legitimacy, as the cases of Muhammad Reza Shah in Iran, the governing elite in South Korea, and the royal House in Saudi Arabia demonstrate (see Binder 1962, and Razi 1981a, 1981b, 1987 on Iran; Hudson 1977 on Arab states; see also el-Fathaly and Palmer 1980 on Libya). This is why Verba, an exception among behaviorally oriented students of politics, has not only considered the role of legitimacy in his studies of Western democracies (e.g., Almond and Verba 1963) but has also noted that the simultaneous existence of a problem in legitimacy and a problem in performance "characterizes most of the 'crises' " of the Third World: "Successful performance in any of the other areas depends on the legitimacy of the government, and the legitimacy of the government depends *to some extent* upon its performance in other areas" (Verba 1971, 315–16; emphasis added).

Fifth, the legacies of Karl Marx and, to a lesser extent, of Max Weber have also exacerbated the previous problems. For Marx morality and legitimacy are simply the product of the economically dominant class and its political agent, the state (Dahrendorf 1959, 14). As parts of the superstructure, they are not meaningful variables (for a critique of later Marxist theories, see Binder 1986). Even Weber's pioneering works do not clearly posit force and legitimacy as two interdependent variables at a given time. Instead, they are primarily concerned with different modes of legitimation (Weber 1948, 1954; for refinement of Weber's typologies, see Rustow 1967, 149–69; 1970a, 15–20). Weber's conceptualization of the state as "the sole source of the 'right' to use violence" (1948, 78) and "a human community that (successfully) claims the *monopoly of the legitimate use of physical force*" (1954, 324–28) led Bendix to conclude that "Weber did not use authority as a separate technical term, but appeared to think of it as a synonym for 'domination' " (1960, 296; on coercion and values, see Johnson 1982, 16–40; Parsons 1964).[5]

Sixth, there has been some failure in recognizing the significance of beliefs and higher values to the actual operation of rationality itself. This has been the case despite substantial literature on the logic of inquiry (e.g., Lakatos 1973; Kuhn 1962), on paradigm incommensurability (Alker and Biersteker 1984), and on cognitive theory, particularly in perception and attribution research, which demonstrates the effects of beliefs, motivation, and information not only on each other but also on perceived causes and, consequently, on expectation and behavior (for synopses, see Harvey and Weary 1984; Kelly and Michela 1980; Perlmuter and Monty 1979; for empirical verification in political science, see, e.g., Heradstveit 1979; Jervis 1968, 1976).[6] In fact, Jervis has concluded that beliefs are both "necessary and dangerous"—necessary to the formation of explanatory theories and dangerous because they tend to lead scholars and decision makers "to err by being too wedded to the established view and too closed to new information" (1968, 459).

Seventh, Western political systems and their social and intellectual bases are on the whole more oriented toward rationality and appreciation of performance factors than are those of the Third World. Consequently, skewed explanations based on projection have at times been the result. Furthermore, while there is nothing wrong on a theoretical basis with such assumptions of rational choice theorists as "equal rationality" of all actors (Bueno de Mesquita 1981; Riker and Ordeshook 1973; Zagare 1984), reflected in the "if-they-can-figure-it-so-can-I-and-vice-versa" syndrome, or voters who assign particular utility percentages to different candidates on the basis of salient issues (Downs 1957), serious problems emerge when one becomes interested in the empirical world. Substantial evidence for variance in rationality has been provided for some time by psychological studies of politics, buttressed by such diverse recent works as Quattrone and Tversky 1988 on political candidates and referendum issues in the United States (which demonstrates the inadequacy of rational choice assumptions) and Razi 1988 on the war between Iran and Iraq (which suggests that what passes as the "rational choice" may be the North American, or "my rationality," theory). Lest analysts totally dismiss the role of myths and become too convinced of the pervasiveness of rationality in the U.S. system, it may be useful to refer not only to recent election campaigns and their results but also to the immense influence of such individuals as the Reverend Josiah Strong, who maintained in 1855 that the United States represented "the purest form of Christianity" and possessed "peculiarly aggressive traits calculated to impress its institutions upon mankind"; President McKinley, who stated that his divine instructions included a specific directive from the Lord to take over the Philippines in order "to educate the Filipinos, and uplift and civilize and Christianize them" (Rainey 1975, 31); and President Ronald Reagan, who exclaimed in 1982 "that this anointed land was set apart in an uncommon way, that a divine plan placed this great continent here between the oceans to be found by people from every corner of earth who had a special love of faith and freedom" (Kegley and Wittkopf 1987, 575). This melange of nationalism and religion has found even a more unusual expression in President Theodore Roosevelt's "peace of righteousness," which divides the world into civilized states (centered around a "virile" America) and barbarous states (with which wars were "most regrettable but necessary international police duty which must be performed for the sake of the welfare of mankind") (Rainey 1975, 35–36). The mode of reasoning is not substantially different from the seventh-century Islamic division of the world into *dar al-Islam* ("the realm of Islam") and *dar al-harb* ("the realm of war"), although no Muslim head of state in modern times, including the Ayatollah Khomeini, has gone so far as Theodore Roosevelt, who also maintained that a "just war is in the long run far better for a nation's soul than the most prosperous peace obtained by acquiescence in wrong or injustice" (Rainey 1975, 36).

Finally, methodological and operational problems have also led to the scarcity of behavioral and rational choice studies of legitimacy in the Third World. Lack of adequate familiarity with the languages and cultures of the Third World, the difficulties of finding access and carrying on in-depth interviews, and the years of investment in time and resources required have resulted—when added to the conceptual problems—in apprehension among Western analysts about dealing with legitimacy and have argued persuasively in favor of economic factors and instruments of coercion (where relevant numbers are more readily available) and of guessing other peoples' behavior on the basis of projection, at times from thousands of miles away.

To improve our understanding of the politics of the Third World, and indeed even of the First and the Second, however, it is necessary to supplement the already-impressive accomplishments of Western scholars and analysts by undertaking further investigation of the clearly more difficult phenomenon of legitimacy. Without such investigation, it would be difficult to understand fully the instability of such regimes as those of the shah in Iran, Chung Doo-hwan in South Korea, Ferdinand Marcos in the Philippines, and Chiang Kai-shek in China or the inability of superior economic and military resources of the United States and the Soviet Union to achieve their objectives in Vietnam and Afghanistan respectively. In fact, the issue of "resolve," which is usually offered as a post facto explanation for such inability (Razi 1985, 53), and "willingness to suffer" (Rosen 1972) also requires an understanding of the role of legitimacy (Azar and Moon 1988). Even such issues as the United States' ability to compete with Japan and West Germany in the international market or the creation of a more orderly and less violence-prone international environment are unlikely to be satisfactorily addressed without some attention to the role of the appropriate values. As Chalmers Johnson has proposed, "the homeostatic capacities of a system will be determined by value sharing *and* by the potency of these values with respect to a given environment" (1982, 55-56).[7] To encourage more attention to this subject, particularly among students of the Third World, and to facilitate its more systematic studies in the future, I offer a preliminary investigation of the sources of legitimacy norms and values in the Middle East.

Religion and Nationalism as Sources of Legitimacy

The primary sources of legitimacy norms in the Middle East are traditional religions and nationalism. There are, of course, other sources, for instance, liberal democracy-constitutionalism; socialism; and Marxism in Soviet, Chinese, and native varieties—but they are at best secondary. Religion and nationalism are also the sources of macroloyalty because they generate the widest bonds of commonly held values in the region. By contrast, the family, clan, section, group, association, and occupation constitute sources of microloyalty, which in the absence of shared religious or nationalist values may generate destructive discord and conflict. The issues of class and ethnicity deserve a brief mention. While the use of class as a conceptual category has an illustrious intellectual tradition, one needs to be careful not to engage in reification without adequate evidence of relevant syntality, particularly in view of the influence of the assumptions of Marx and Engels. All available evidence on the Third World so far indicates that class affinity is far weaker as a source of cohesion and commitment than are religion and nationalism.[8] Ethnicity, on the other hand, can move in either direction. It may become converted to nationalism, but this is by no means automatic. The ethnic Poles and Italians in the United States are ardent U.S. nationalists, just as the ethnic Qashqa'i and Azerbaijani Turks are ardent Iranian nationalists. On the other hand, some ethnic Kurds in Iraq, Turkey, and Iran seem to be in the process of forming Kurdish nationalist loyalties, as did many ethnic groups in the Ottoman empire during the nineteenth and twentieth centuries.[9]

The three great monotheistic religions of the world—Judaism, Christianity, and Islam—as well as dualistic Zoroastrianism, originated in the Middle East and continue to shape its prevailing norms and values. There are Christian minorities in Egypt, Lebanon, Syria, Iran, and other countries at present; and Judaism has been a major factor in the formation of Israel's political culture. But Islam retains its overwhelming predominance. Literally tons of writings on Islam have been available for a long time in the Middle East and the West, which in the larger sense includes the USSR and its allies. In view of the fact that one in every five inhabitants of our planet is Muslim, recent resurgence of Islamic symbols and behavior on a large scale in the Middle East and the fall of the Shah's regime and establishment of the Islamic Republic in Iran in 1979 have rekindled Western concern and apprehension and generated rather rapidly a new corpus of literature under the rubric of "Islamic fundamentalism."[10] While treating this literature and the fine points of Islamic jurisprudence are beyond the scope of this paper, six points are germane to its focus.

First, Islam has influenced political attitudes on such matters as collective identity, the concept of justice, the nature of the legitimate political system, the rights and obligations of ruler and ruled, and the kind of characteristics decision makers should possess. It has a wider and deeper domain than nationalism, particularly among the lower and lower middle strata, which constitute the overwhelming majority of the population. Even among the more or less Westernized segments, who have readily accepted a number of Western innovations in the areas of technology, bureaucracy, military organizations, communication and transportation, resistance and rebellion have not been rare when it comes to accepting Western ideas and behavioral patterns that were perceived as alien to the core of Islamic culture (see Razi 1981b). In general, borrowing in the sphere of performance, being more readily subject to universal rational criteria, has encountered considerably less resistance than borrowing in the sphere of values. This is clearly demonstrated by the wide adoption of telephones, automobiles, commercial jets and even computers in contrast to the persistence of traditional Muslim attitudes concerning the status of women, the expectation of piety in political leaders, and the family's duty to care for the aged. A similar contrast would be characteristic of a number of Israeli Jews.

Second, Islam created a religion and a state at the same time. Its constitutional theory stands in sharp contrast to that of Christianity, which is represented by the doctrine of the two swords. The separation of the spiritual and temporal realms, propounded by Saint Augustine and Saint Thomas Aquinas, simply provides further explication of the Bible's "Render unto Caesar the things that are Caesar's and unto God the things that are God's." The many centuries of multiple loyalties and obligations in Western Christendom provided for a different psychological-moral orientation to politics and socialized the individual to the ability to adjust to the changing requirements of various roles in, for example, family, political party, church, and business or profession. The required adjustment to various roles has, of course, not been without its own creation of aliention and moral problems. By integrating political and religious obligations, however, the Islamic culture has provided less incentive and opportunity for such socialization. Furthermore, these differences have meant that political obligation covers a limited realm in the West and is entitled only to its share of loyalty and no more, even when political authority *is* perceived as legitimate—whereas, among the Muslims a legitimate political ruler is more likely to evoke total devotion and supreme sacrifice, since the source for authority is God. By the same token, a Muslim has a greater obligation to disobey and revolt against illegitimate rulers and institutions, particularly when a legitimate alternative is available and the Islamic society is not threatened by prospects of long-term disorder and civil strife.[11]

Third, the nature of religious legitimacy has implied the recurrence of divergence between what Dekmejian (1985a) and Lewis (1985) characterize as "establishment Islam" on the one hand and "popular Islam" on the other (Bill 1984 uses "populist Islam"). In an Islamic society, political leaders have generally found it wise to profess and sponsor religion regardless of the depth of their own belief and commitment. When the establishment version diverges from popular Islam, there is great potential for instability and revolution as well as the emergence of such leaders as the Mahdi in the Sudan, Hassan al-Banna in Egypt and the Ayatollah Khomeini in Iran. Western imperialism, materialism, and secular ways have thus been detrimental to many existing regimes, particularly when such regimes could not successfully legitimize themselves on the basis of nationalism either. Consequently, nativistic religious movements that have sought to end the direct and indirect alien sway over the lands of Islam and to return to an earlier ideal and idealized set of attitudes and institutions have elicited wide emotional support since long before the Iranian revolution (see Dekmejian 1980, 1–3; 1985a, 9–19 and chaps. 6–10). Of course, when the establishment and popular versions converge into one in the eyes of the believers, great devotion to the state is the likely outcome, as the regime of Khomeini, particularly in its first few years, has amply demonstrated.

Fourth, Islam has not been either completely monolithic or rigid and static. It has had different sects and variations in a number of religious practices and institutions (see al-Baghdadi 1919, 1935; al-Shahrastani 1842–46). Rodinson (1978) provides sufficient evidence to demonstrate that contrary to the prevailing impression in the West, Islam was not an impediment to capitalism or socialism (and has in fact displayed a good deal of dynamism in the area of economics) any more than the existence of traditional rituals and religious attitudes among the Jews in general served as a major impediment to the emergence of sophisticated commercial and entrepreneurial enterprises or impressive scholarship, despite the adversity of the Diaspora. In fact, the Renaissance, the Enlightenment, and the Industrial Revolution in the West owe their intellectual origins and a good deal of their initial impetus to the works of the Muslims of the medieval period on mathematics, philosophy, chemistry, physics, and medicine that were made available to Western Europe through the conduit of Spain, particularly the city of Toledo, in translations by Jewish scholars. Furthermore, Islam has not been opposed to the development of representative government, as the cases of Iran, Kuwait, Turkey, and Egypt to varying degrees demonstrate. That the same is true of Judaism is even more evident, given the state of Israel.

In any case, politically, the model for the Islamic nativists, or fundamentalists, is usually the constitution of Medina after the hejira (A.D. 622, the beginning of the Islamic calendar). A more broadly constructed model, rejected by the Shiites, includes the period of the "first four pious caliphs," or the golden age of Islam (A.D. 632–61). Islamic constitutional theory, however, has developed along three different lines since that period, particularly after the battle of al-Siffin (A.D. 657) between Caliph Ali, the cousin and son-in-law of the Prophet Muhammad and the first imam of the Shiites, and Muawiyya, the rebellious governor of Syria and founder of the Umayyad Dynasty (A.D. 661–749), and the failure of subsequent attempts at arbitration. The three lines depart from the constitutional positions assumed by the Sunnis (traditionalists), the Kharijutes (seceders, foreigners), and the Shiites (partisans) during the first civil war. The premise that in a state founded on religion the rulers should be religious characters has led the Sunnis to the glorification of the first four caliphs. In order to reconcile their religious ideals with the cruel facts of their historical experience, some Sunni ulema (religious scholars) have since upheld the duty to obey political rulers as long as they profess Islam and manage to maintain order, a ruling that formed the genesis of "establishment Islam." The resulting identification of the ulema with the rulers, exacerbated by their lack of independent sources of income, accounts for the fact that Sunni fundamentalist leaders, in contrast to those of the Shiites, have been lay religious writers and activists (for elaboration, see Dekmejian 1985a, 1985b; Sivan 1985). But pious Sunni religious leaders have always warned the believers against political rulers, a warning that reflects the grounds for "popular Islam."[12] Consequently, Sunni constitutional theory has not, historically, developed a clear and coherent theory of political obedience.

On the opposite sides of the Sunni position have been the Kharijites and the Shiites. The Kharijites withdrew from the rank of Ali's supporters during the civil war and regarded both Ali and Muawiyya as violators of the laws of God, motivated by considerations of personal power. They maintained that political leadership should be vested in the most pious Muslim by the free choice of the *ummah* (community of believers). From the doctrine of free choice and such Qur'anic statements as "Verily the most honored of you before God is the most righteous of you" (al-Hujarat, 13) they also derived the logical, egalitarian conclusion that succession was not limited to any clan or ethnic group and that a black slave was as entitled to leadership as the members of the house of the Prophet. In their view, the leader was required to submit to the will of God and strictly conform to His laws, as were the members of the community. If the leader did not fulfill his obligations, he could and should be deposed by the community. Those who even contemplated commitment of great sins were sinners and infidels (Goldziher 1920, 160; Macdonald 1903).

While the Kharijite position culminated in puritanical republicanism, the Shiites developed the doctrine of divine succession through the house of Ali. Depending on which of Ali's descendants they have since accepted as their imams (spiritual and legitimate secular, though not necessarily actual, leaders), the Shiites have developed additional sects, of which the Twelve Imamis (Ithna Ashariya) of Iran are by far the largest and most significant. The period of the "historical *imamat*" ended for the Twelve-Imamis Shiite sect with the disappearance, or occultation of the Mahda (Messiah) in the ninth century. The period of Ghaybat-e Sughra (Lesser Concealment), in which the Mahdi was believed to have been represented by agents, lasted for 69 lunar years. From 940–41 started the Ghaybat-e Kubra (Greater Concealment), in which no one has direct access to the hidden imam. This is the age in which we are living. Only the consensus of the *mujtahids,* the highest-level theologians capable of the practice of *ijtihad* (fresh religious interpretation), is supposed to reflect the will of the hidden Twelfth Imam. Until the reappearance of the Mahdi all government is by definition a usurpation. So far as the duty to obey secular rulers is concerned, the Shiite theory has at times shown some of the same variations and uncertainties as have the Sunnis. But the commonly accepted legitimacy theory of the Shiites before the advent of the Ayatollah Khomeini and the Islamic Republic in Iran was formulated in a treatise written in 1908 by Na'ini (1979) and endorsed in the preface by the Ayatollahs Muhammad Kazem Khorasani and Abdullah Mazandarani, the two *maraje'e taqlid* (sg. *marja',* a *mujtahid* selected by the believers for emulation in matters of religion), existing at the time.[13] Based on such Qur'anic commands as "consult them in affairs" before making a decision (Al-Imran, 159) and that Godly and everlasting matters are for those "who (conduct) their affairs by mutual consultation" (Shura, 36–38), this position maintains that a government based on representation and consultation is the least usurpatory and most legitimate in the absence of the imam of the age, while dictatorship is the most usurpatory and illegitimate. It is also significant to note that a later authoritative book written about *rouhaniyyat* (spiritual leadership) and the position of *marja'* (Tabataba'i et al. 1962) did not even consider Khomeini's (1971) *velayat-e faqih* (government by a theological jurist—in this case himself) as a subject for discussion (see also Akhavi 1980).

Fifth, Islam has also been characterized by a substantial amount of ossification and rigidity, particularly in some of its laws and rituals. This is particularly important if a political system based on this religion is to survive and function in the contemporary world. Viability and stability, that is, the management of orderly change (not the maintenance of the status quo, which is often confused with stability), require sufficient synchronization between the environmental changes of a political system and its relevant orders and institutions or at least the ability to avoid substantial and unsettling incongruities. For example, when environmental conditions change because of scientific and technological discoveries and innovations and when new laws are needed or old ones require modification, persistent noncongruence poses threats to stability (Johnson 1982, 26–27). Perceptive analysts of comparative politics have investigated the problems posed for increase in performance by some traditional orientations and practices in the Third World (e.g., Pye 1971, 138–39). But comparable problems exist in the modern or postindustrial systems as well. The pluralist ethos combined with intense individual and group competition on the basis of parochial interests in the United States, for example—sanctioned by a leap of faith in Adam Smith—seem clearly less functional to productivity than the disposition toward cooperation between management, labor, and government in Japan, a legacy of feudal traditions (see Vogel 1979, 27–157; Ward and Rustow 1964, 445–51). In Islam the role of *ijtihad* becomes vital in this connection. *Ijtihad*, however, had been closed to the Sunnis since the ninth century, after the four Sunni schools of jurisprudence (Maleki, Shafi'i, Hanafi, and Hanbali) had already been established; and the Shiites have construed it too narrowly. Viability of any Islamic state necessitates a return to the more liberal usage of *ijtihad* as neither fundamental religious tenets nor logic bestow the ninth century with peculiarly divine characteristics. In fact, reopening *ijtihad*, with occasional supporters among Sunnis in the past, has recently generated a growing interest among the ulema and even Sunni kings (for references, see Moursi Badr 1987).

Of course, other methods of bringing about change have been available to Islam. One method, albeit less institutional and predictable, has been through the rise of such charismatic and popularly based religious leaders as Khomeini. He has managed to bring about a number of changes in the traditionally established Shiite precepts, any of which would have been regarded as *bid'at* (religious invention) and subject to charges of infidelity and excommunication in ordinary times. These include his introducing *velayat-e faqih* (partially taking away the well-established freedom of the Shiites individually to select their own *marja'e taqlid*), forcing them to follow his personal dictates not only in religious but also in political matters, and even accepting the title of Imam. When pressed on this issue, some of Khomeini's apologists retort that what they really mean is imam or *na'ib* (agent of the Imam). It would be interesting to ascertain under what set of circumstances the Mahdi granted Khomeini any more than any other *mujtahid* (who is superior in religious knowledge to a jurist) the function of a *na'ib*, a development that would change the nature of the Greater Concealment. Furthermore—whether consciously borrowed or not—some of Khomeini's innovations are clearly more akin to the ideas of the Kharijites than those traditionally associated with the Shiites.

The culmination of Khomeini's innovations in Islamic constitutional theory occurred in 1988 when, through a letter written to President Ali Khamene'i and widely disseminated by the mass media, he overruled the president's reiteration in the Friday Prayer at the University of Tehran of the hitherto unchallenged tradition that Islamic "government must act within the framework of God's rules." Instead, Khomeini asserted that "government, which is a derivative of the absolute tutelage of the Prophet of God, God's blessing upon Him and His House, is one of the primary rules of Islam and superior to all *far'iyah* [derivatives, secondary rules], even to those concerning prayer, fasting and hajj" (Khomeini 1988).[14] Consequently, the Islamic "government is authorized unilaterally to abrogate all legitimate contracts it has concluded with the people anytime it judges that they are contrary to the welfare of the country or Islam. It can also prevent the occurrence of any matter, whether in the sphere of religion or otherwise, as long as it deems such matter inexpedient for Islam" (pp. 1–2). He has since personally instituted a new Council of Expediency to overrule the constitutional Council of Guardians (vested with the function of ensuring that laws passed by the Majlis ["Assembly"], do not contravene Islam), personally deposed the Ayatollah Hossein Ali Montazeri (elected by the constitutional Council of Experts) as his successor, and has even been declared by his supporters to be "above the constitution." These developments are remarkable, if not astonishing, when one considers not only the well-established Shiite theory and practice but also the position taken earlier by Khomeini himself stating that "no *faqih* [theological jurist] has said, nor any [religious] book has written that we [*faqihs*] are Shahs or that kingship is our right" (Khomeini [c. 1944], 186; see Khomeini 1978, 369; for a comprehensive treatment of the evolution of his position, see Razi 1988, 702–3); that the function of *faqih* is the execution of the laws (1971, 56–57); and that a "*faqih* . . . does not possess *velayat* [tutelage] over other contemporary *faqihs* or the power to appoint or dismiss them" (1971, 57).

Whether these innovations, particularly the theory of absolute government and *velayat-e faqih*, are likely to survive Khomeini and be institutionalized is uncertain. Whatever the fate of any of Khomeini's specific personal innovations, however, the most significant aspect of the Islamic Republic may well consist (1) of changing the thrust of Shiism from quietism to activism-militancy, mainly by reformulating the role of *shihadat* ("martyrdom") and combining it with *jihad* (crusade to defend and extend the realm of religion) and abandoning the pessimistic characteristics of Shiite Messianism;[15] and (2) of slowly but surely resorting to adaptation and innovation in Shiite traditions by adopting such new laws and practices as interest on loans and modern taxes (instead of the only hitherto legitimate religious taxes of *zakat* and *khums*) that form the basis for the everyday administration of a state's affairs. These imply that Shiism in particular and, by the force of example, Islam in general will probably never again be quite the same as they were during the last few centuries.

Another important method of bringing about change in Islam has been reformism. It is represented by such diverse works as those of Jamal al-Din Asadabadi al-Afghani (Keddie 1968, 1970, 1983), Muhammad Abduh, Rashid Rida, and al-Kawakibi (Gibb 1972; Hourani 1962; Kerr 1966), Bazargan (1976, 1982–83) and Shariati (1973, 1978, 1979). It has had an intermittent but limited success. Nevertheless, its cumulative effect may prove more significant in the long run because, unlike fundamentalism, it seems to have recognized and groped for synthesizing the politically requisite but not easily reconcilable roles of faith and reason. Regardless of its long-term utility as a legitimizing myth, however, only Islamic fundamentalism has so far succeeded in providing an unprecedented level of innovation and adaptation and imputing "a deep sense of spiritual belonging and brotherhood to millions of alienated Muslims" (Dekmejian 1985a, 176).

Moreover, despite being a religious leader and substantially unfamiliar with the intricacies of technology, economics, and international relations, Khomeini has not refrained from the use of either incentives (when expedient) or a high level of coercion against actual or potential challengers (see Razi 1988). These behavioral patterns substantiate the significance of the role of strategy and tactics in the success of Third World leaders, be they charismatic ideologues like Khomeini and Mao Zedong, organizers and conspirators like Lenin, or nationalists like Mustafa

Kemal, the founder of modern Turkey. Consequently, analysts also need to pay some attention to the significance of "social action as a contingent process" (Johnson 1982, 184–86) in order to "guard against two related fallacies. The first is the view that the leader was indispensable to the results achieved. . . . The second is the opposite view, that the result was inevitable, that any other leader would have had to adopt the very same course" (Rustow 1970a, 21).

Finally, while until the Iranian revolution Western orientalists and analysts of the Middle East failed to grasp the significance of Islam because of their self-centered orientation to cultural change and overestimation of the roles of economics and force (for an exception, see Lewis 1976), they have since deemphasized the role of other sources of cohesion and commitment. To redress this imbalance of perspectives, it is particularly important to reconsider the role of nationalism, which had previously received a great deal of attention. Some of the sophisticated definitions of nationalism were provided as early as the nineteenth century (Mill 1939, 206; Renan 1882, 27). Fairly comprehensive bibliographies (e.g., Deutsch and Merritt 1970), general analyses (e.g., Deutsch 1966; Emerson 1960; Hayes 1931; Rustow 1967) and specific works on Arab nationalism (Antonius 1961; Carmichael 1967; Dekmejian 1971; Haim 1961; Nuseibeh 1956), Iranian nationalism (Cottam 1964), Turkish nationalism (Edib Adivar 1930; Gokalp 1958; Lewis 1961; Rustow 1970b; Ward and Rustow 1964) and Israeli nationalism (Gal 1973; Halpern 1961; Herzl 1970) are also available. I will limit myself here to the following five points, which relate nationalism to religion and the politics of the region.

First, although consciousness of similar identity has existed among some people for a long time, modern nationalism began in Europe of the eighteenth century and its guideposts are the U.S. and French revolutions. The latter was more thorough in breaking with the traditional patterns and created a "civil religion" that has since shown a remarkable success in converting the world to its values. It developed the national flag, national anthem, altars to the motherland, and secular baptisms and funerals as new religious symbols and ceremonies to replace those of the old Catholic faith. And nationalists—including thoroughly committed citizen-soldiers who as peasants hid in caves to escape serving in the army only a few years before—began to spread the new faith with missionary zeal (Hayes 1931). Religious qualities of nationalism in the United States have been investigated by such analysts as Warner (1962), Verba (1965), and Cole and Hammond (1974), as have those of such millenarian movements as Marxism in the Third World (Razi 1987, 463–64). Despite decades of socialization to Marxism, nationalism clearly remains a major source of cohesion and cleavage even in the Soviet Union, as demonstrated not only by the recent events in the Soviet Republics of the periphery but also by the fact that the huge statue of the heroine at Volgagrad (Stalingrad) pointing west, whence the invading German armies came before their back was broken in World War II, reflects the sentiment of patriotism rather than that of class consciousness or international communism. In any case, just as the French armies' singing of the "Marseilles" and marching across Europe converted the European peoples to nationalism, Western European imperialism in the Middle East served as a catalyst to hasten its own antithesis, nationalism.

Politically, nationalism's primary goal has been to end foreign domination and to install independent and autonomous rulers who could undertake the necessary social and economic reforms for the advancement of the country. But the most significant, and least studied, aspect of nationalism has been psychological. The legacy of years' subjugation to superior foreign organizations, armies, technologies, and agents by peoples of long histories and great civilizations—despite the usual exaggerations, myths, and historical manipulations—has vested in nationalism the role of resurrecting the dignity of the articulate population and eliminating the feeling of insecurity and inferiority. The appeal of nationalism, just as of religious fundamentalism, in the Middle East is thus to a large extent a reflection of the crisis of dignity, that is, of individuals' sense of self-worth, honor and esteem. It is particularly significant to understand (1) that the territories of many states in the Middle East were created according to needs and perceived interests of the Europeans rather than those of the local populations and (2) that the subjugation was exercised not only overtly through imperialism but also covertly through such nominally autonomous but actually subservient elites as Nuri Said in Iraq, Sayyid Zia' Tabatabai and Muhammad Reza Shah in Iran and the sheikhs of the Gulf states. This has caused such an enduring perception of regional leaders' subservience to foreign governments, strengthened periodically by the revelations of the works of foreign intelligence agencies or inadvertent confessions of such rulers as Muhammad Reza Pahlavi (1980), who complained about not being given unequivocal instruction through the U.S. and British ambassadors about what to do during the Iranian revolution, that a number of educated even in Iran of today seem quite convinced that the Khomeini regime was installed by the United States.[16] This perception also constitutes the main psychological reason for the compelling need of even politically autonomous leaders to go to the extremes to prove to their people and perhaps to themselves beyond the shadow of a doubt that they are not subservient to great powers.[17] It should be added that in the case of the state of Israel, which has not suffered foreign dominance since its creation, the legacies of Diaspora and holocaust have contributed to an obsessive concern with security. This has in turn contributed to the more-than-isolated instance of what appears to the rest of the world as highly aggressive uses of military coercion or to such perceived causal structures as those illustrated by Heradstveit's (1979) study of Israeli and Arab subjects, in which each side attributed its own "good" behavior to dispositional factors and "bad" behavior to situational factors, while using exactly opposite attributions for the other side's behavior, further complicating the potential for conflict resolution. There is, of course, a good deal of objective and verifiable ground for security and economic concerns and a large measure of calculating rationality and purposeful propaganda and distortion involved in pressing one's case. But it is untenable to assume that diabolically clever plans and rationally calculated designs form one's own subconscious, higher values, and attributions.

Second, the relationship between nationalism and religion in the Middle East is complex and curvilinear. Both value structures are to varying extents held by an overwhelming majority of the population and generate at times mutually reinforcing, and at other times conflicting, patterns of behavior and institution. Religion has been a major contributing factor in developing and preserving the national identities of the Arabs, the Iranians, the Israelis, and even the Turks (in Turkey). Nationalism and religion have also in many instances served as the basis for cooperation against foreign incursions. Thus, the two sets of orientations may not necessarily clash, but they are not always easily reconcilable either. An Islamic state based on the *ummah,* like a Jewish state, is basically multiethnic; and national differences are at best secondary, if not irrelevant. The difficulty of reconciling the two bases of state formation is historically observable in the breakdown of the Ottoman Empire and currently in the interpersonal and intergroup conflicts among the more religious and more secular citizens of Israel as well as the Jewish citizens of other states. While in the Ottoman instance nationalism was the epitaph to a centuries-old religious state, in Israel's case religious affiliation set in motion nationalism, which in turn has in some instances led to conflict between the two sets of affiliations. The case of Lebanon on the other hand reveals how religious differences, reinforced by vertical confessional structures and parochial leaders (*zu'ama*), can drastically hinder the emergence of nationalism and disrupt the state (see Binder 1966; Azar and Marlin 1987).

Third, nationalism has so far shown more capability to acquire modern technology and science, higher levels of economic growth, and advanced means of defense. But this by no means presages the future. Islam not only remains more deeply rooted, has wider adherence, evokes considerably greater commitment, and seems to be in ascendance but is also at least potentially capable of directing this commitment toward higher performance in science, technology, and economics.

Fourth, insofar as the ebbs and flows of nationalism and Islam are concerned, it should be noted that nationalist Mustafa Kemal became supreme in Turkey—despite religious forces—and Turkish nationalism has so far shown no sign of succumbing to religious fundamentalism (Rustow 1987, 72–83). Reza Khan made a compromise with religious leaders initially and abandoned efforts to make Iran a republic but eventually dominated the clergy as a shah on the basis of nationalism. Gamal Abd al-Nasser overcame the Muslim Brotherhood in Egypt, because there was little doubt among the people about his integrity as an Arab nationalist. Hafiz al-Asad has been able to govern Syria as a member of the minority Alawites and despite opposition by the fundamentalists headed by Muslim Brotherhood, partly because he is perceived as the only Arab leader capable of standing up to Israel and the United States and thus resurrecting some measure of Arab dignity. It is also noteworthy that Nasser and Asad have been more strongly committed to *qawmiyyah* (Arab nationalism) than to *wataniyyah* (Egyptian or Syrian nationalism). Although at this point the prospects for Arab national unity do not appear promising, it would be erroneous to underestimate the appeal of Arab nationalism or dismiss its long-run chances. Just as there is an establishment and a popular Islam, there is also an establishment Arab nationalism (reflected in the lip service paid to the concept by pro-Western conservative regimes who in fact prefer *wataniyyah)* and a popular one (being observable particularly among the middle classes and educated segments who comprise most of the modern specialists in persuasion and organization). Furthermore, it is clear that such *wataniyyah* allegiance as Saudi Arabian, Jordanian, and Moroccan nationalism is not—and is unlikely in the foreseeable future to become—sufficiently robust by itself to withstand the onslaught of Arab nationalism or Islamic fundamentalism. It should also be emphasized in this connection that the defeat of the shah's regime is more significant for demonstrating the role of legitimacy than for indicating the comparative viability of nationalism and Islam. The shah was never perceived as an autonomous ruler by the Iranian nationalists, as a constitutional leader by the majority of the constitutionalists, or even, in the end, as a religiously tolerable monarch by a substantial number of Shiite believers. It was primarily the failure of the regime in these three different but converging grounds of legitimacy—religion, nationalism, and constitutionalism—that brought the shah and the monarchy down, not its performance in the areas of economics technology, administration, organized instruments of physical force, or even foreign policy (Bill 1982, 1988; Keddie 1981; Razi 1981a, 1981b, 1987). Furthermore, the shah was unable to recruit even 10 volunteers who would be willing to give their lives at his commands despite all the wealth at his disposal. Khomeini, on the other hand, has been able to recruit willing martyrs by hundreds of thousands despite substantial economic reversals suffered by the Iranians since the inception of his regime. The difference demonstrates the potential motivating power of beliefs as compared to interests.

Finally, in light of the appeal of Islamic fundamentalism and Arab nationalism, prospects do not appear promising for either the United States' position in the Middle East or the legitimacy of the friendly Arab regimes, despite a number of complementary economic and security interests. Several factors are particularly relevant to this assessment. The first is the seemingly unconditional support for Israel, particularly as manifested in the prejudicial definition and policy formulation on such matters as human rights, nuclear nonproliferation, security, terrorism, torture, and legal title to land. The second consists of support for conservative regimes perceived as subservient to the West and unconcerned with the aspirations of their own population. And the third is the diffusion of U.S. values and patterns of behavior perceived as alien to Islam and detrimental to nationalism and genuine political autonomy. The first of these is by far the most significant, as it has been detrimental to the legitimacy of friendly Arab governments and served as an impetus to "radicalization" of the area.

It is still possible that regimes perceived as genuinely nationalist may in the future prove more successful than those based on Islamic fundamentalism. In fact, there has already been a partial return to such symbols of nationalism as *millat,* rather than *unmat,* even in the mass media and speeches of the leaders of the Islamic Republic, particularly since the end of the Iran-Iraq War.[18] But the degree to which the peoples of the Middle East measure such genuineness will depend to a great extent on the ability of such regimes to oppose the U.S.-Israeli positions, particularly concerning the status of the Palestinians. The governments of Saudi Arabia, the Gulf states, Jordan, Tunisia, and even Egypt thus appear in trouble on grounds not only of religious revivalism but also of Arab nationalism. Some respite for the West was provided by the Soviet bungling and military intervention in Afghanistan, which exacerbated Soviet disadvantages stemming from the nature of its values and weaknesses of its technological and industrial sectors. Conditions in the Middle East have for some time been most promising for Japan and Western Europe. A solution to the Arab-Israeli conflict, particularly to the Palestinian problem and the holy shrines of Jerusalem, will, of course, substantially change the nature of these equations.

Conclusion

Conceptually, the most significant factors accounting for the partial or total neglect of legitimacy by many contemporary studdents of politics seem to have consisted of the absence of adequate appreciation for (1) fundamental difference between the nature and sources of legitimacy and those of performance; (2) the role of values in overcoming the inevitable incongruities that arise between the individual's rational search for interests and the collectivities' survival requirements; and (3) the interdependence of legitimacy and performance. Methodologically, projection, the assumption of equal rationality of all actors, inadequate familiarity with the cultures of the Third World, and the difficulties of finding access and administering interviews have been important.

Although Islam has been characterized as somehow incongruent with technological innovation and the other performance requisites of an economically developed system because of ossification in its laws and rituals, it has on the whole been neither monolithic, rigid, and static nor an impediment to the development of capitalism, socialism, or representative government. It has in fact generated an array of fairly sophisticated constitutional theories as well as the practice of *ijtihad*, which, if fully utilized, is a significant instrument for continuous adaptation. It has also been shown that contrary to the prevailing impressions in the West, Islamic fundamentalism of Iran has provided a major source for innovation and adaptation in Islam and generated a profound sense of spiritual gratification and brotherhood among the masses of alienated Muslims.

The relationship between nationalism and religion is complex and curvilinear. It may, therefore, be premature to rule out the viability of nationalism in favor of religious fundamentalism, despite the latter's recent achievements. In this connection, a great deal will depend on the nature of the decisions made by the respective nationalist and religious leaders and the operation of external factors over which they have little control.

Notes

1. Even the appropriateness of majority rule—reflected in the superior quality of the candidate receiving 51% over the one receiving 49% or candidates winning in such single-member-district-plurality-vote electoral systems as the United States and Britain—is by no means self-evident. Rather, it is primarily the product of conventional acceptance and socialization.

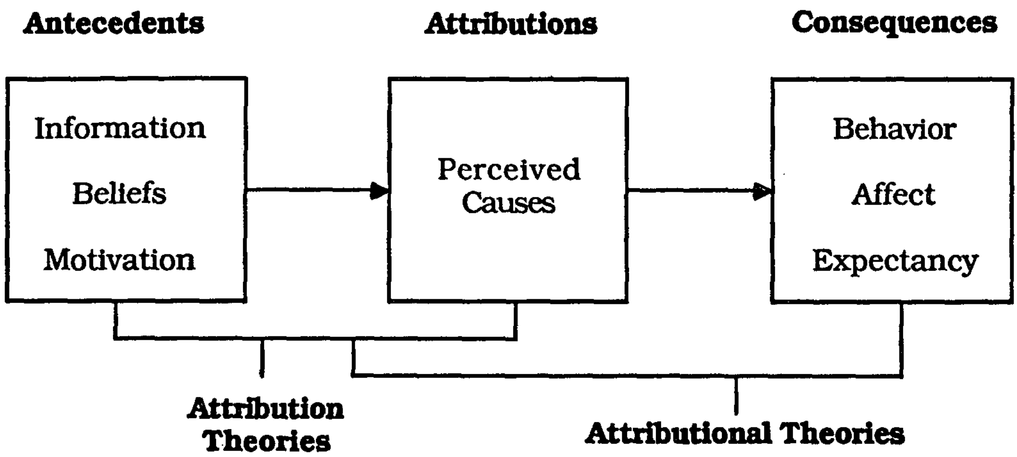
2. The significance of perception is not limited to legitimacy. As Thomas and Znaniecki noted long ago, "If men define situations as real they are real in their consequences" (1918-20, 81). Timasheff developed this theorem further and stated that "behavior can be understood only when it is studied within its whole context—the situation not only as exists in verifiable, objective form, but also as it *seems* to exist to the person himself. The latter subjective factor must never be discounted in the social analysis" (1957, 151).

3. More importantly, politics is very different from economics: first, because of the role of higher values and legitimacy in the former; second, because the purpose of political leaders is not to serve self-interest rationally but to serve collective interest; and, third, because political power is situational-relational and nonfungible in contrast to money in the economic realm (see Baldwin 1979). Furthermore, there are difficulties involved with the assumption of rational utility maximizers even in economics (See Simon 1955, 1985). That actual behavior shows variance in rationality is well known to advertisers, firms, and politicians who resort to propaganda, paying millions of dollars to well-known actors to cause the target groups to purchase insurance contracts of clearly dubious value and billions of dollars of worthless cold remedies, purchased not because of their actual utility for the achievement of the targets' goals but because the targets like and "trust" the employed actors. Moreover, even in the same country the rationality of one economist seems substantially different from that of another, as the debates about deficits in the U.S. budget and balance of trade have amply demonstrated. It seems that a study of beliefs, motivations, and information of the economists provides a more reliable guide to their preferences than does the assumption of equal rationality.

4. Otherwise, saints and suckers become identical, greed becomes contagious, and morality and ethics (including professional ethics) become synonymous with stupidity.

5. Analysts have read Weber's writings on legitimacy differently (see Connolly 1984, 9-12).

6. The following figure provides a summary of attribution theories.



Source: Kelly and Michela 1980; for a synopsis of the literature on measurement techniques and findings in attribution research, see Harvey and Weary 1984. For debates, see Ross 1977; Quattrone 1982; Harvey, Town, and Yarkin 1981; Funder 1982.

7. This reflects the significance of what anthropologist Marvin Harris calls "the culture/nature interface." But the following argument seems to misunderstand the role of social sciences: "To endow the mental superstructure with strategic priority, as the cultural idealists advocate, is a bad bet. Nature is indifferent to whether God is a loving father or a bloodthirsty cannibal. But nature is not indifferent to whether the fallow period in a swidden field is one year or ten" (Harris 1979, 57). But reification of nature is not a good bet either, and, unless there are social systems, we cannot understand these matters, engage in environmental engineering, or even care.

8. In the case of the Iranian revolution, for example, opposition to the Shah cut across all social strata (see Bill 1982; Razi 1987). In fact, in his earlier work, done during the Shah's regime, Bill (1972) found that the middle class reflected highly divergent orientations and ideologies. The findings certainly do not sustain the existence of class consciousness in any significant way.

9. Powerful ethnic affinities may become a major barrier to the development of nationalism as a legitimizing basis for the formation of viable states, as can be seen in Africa and parts of the Middle East. Vestiges of ethnic separatism are still observable in the Soviet Union, Canada, and even Britain.

10. The terms *nativism* and *atavism* seem more appropriate for this purpose. But we have retained *fundamentalism* because of its now common usage, despite the fact that some of its aspects "are more reminiscent of liberation theology in Latin America than they are of Christian fundamentalism in the United States" (Munson 1988, 3). For a sample of works in this area, see Akhavi 1980; Amir Arjomand 1984; Cole and Keddie 1986; Dekmejian 1980, 1985a, 1985b; Esposito 1983; al-Gazzali 1953; Husaini 1956; Keddie 1981; Kerr 1966; Khomeini 1971; Mawdudi 1980; Pipes 1981; M. Qutb 1979; S. Qutb 1975, 1980, n.d.; Sivan 1985.

11. While the Sunnis have accepted the need for avoiding protracted conflict and civil strife, the Shiites have developed the doctrine of *taqiyyah* ("dissimulation"), which authorizes the believers to pretend to other than their true beliefs when threats to their lives and possessions are substantial and the prospects for overthrowing an illegitimate regime are inconsequential.

12. From the perspective of fundamentalism, the most important Sunni religious leader has been Ibn-Taymiyyah (d. 1328). His neo-Hanbali writings (e.g., Ibn-Taymiyyah 1923-30) have constituted the main source for the inspiration of such authors as Mawdudi (1980), which in turn influenced M. Qutb (1979) and S. Qutb (1975, 1980, n.d.). In fact, the notion of a new *jahiliyyah* ("barbarism"), was originally used to describe the conditions of Arabia before the rise of the Prophet Muhammad and now used by the Sunni fundamentalists (e.g., S. Qutb n.d., chap. 10) to characterize "the evils of capitalism" and "the materialistic, godless communism," is probably taken from Ibn-Taymiyyah who used it to describe the Mongol reign in the thirteenth century.

13. Khomeini's supporters have deemphasized this position and resurrected the writings of Sheikh Fazlollah Nuri, who favored authoritarianism and turned against the Constitution of 1906 once it became evident that the latter did not mean the insti-

tution of *shariah* (religious rules of behavior).

14. The Shiite popular religious beliefs and practices are divided into *usul* (fundamentals) and *foru'* (derivatives). *Usul* consist of belief in (1) *tawhid* (a unitary God), (2) *adl* (God's justice), (3) *nabowwat* (prophets and Muhammad as the last messenger of God), (4) *imamat*, (the Twelve Imams), and (5) *ma'ad* (resurrection and the hereafter). The Sunni *imans* (primary beliefs) do not as a rule include *adl* and *imamat*. The Shiite *foru'* consist of (1) prayer, (2) fasting, (3) *khums* (an annual tax of one-fifth of net increase in certain items of property to maintain the ordinary descendants of imams, orphans, and the needy), (4) *zakat* (a tax to be paid to the Muslim treasury for the needy, those in debt, and travelers—which in practice was the clergy during the period prior to the Islamic Republic), (5) *amr-e be ma'ruf* (urging to good deeds), (6) *nahy-e az munkin* (injunction against bad deeds), (7) *hajj* (pilgrimage to Mecca—by those who can afford it—to visit the holy stone of Ka'ba at a particular time of the year), and (8) *jihad* (holy war to defend or extend the realm of faith). The Sunni *ibadat* (devotional duties) include all of the Shiite *foru'* except items 3, 5, and 6. Some Sunni scholars place *jihad* under *mu'amalat* (interpersonal relations), which has extensive and mostly similar codes in both sects. Interpersonal relations are divided into (1) *wajib* (mandatory), (2) *mandub* (recommended), (3) *mubah* (permitted), (4) *makruh* (reprobated), and (5) *haram* (forbidden) (see al-Baghdadi 1919, 1935; al-Shahrastani 1842–47; Browne 1959, 381–402; Goldziher 1920; Ibn-Babuwaihi 1959). For one of the most extensive early Western works on *foru'*, see Querry 1871–72. Khomeini (1988) maintains that since belief in the prophets is a fundamental rule, the government of the Islamic Republic, which is basing its tutelage on this rule, is authorized to abrogate the derivatives when it judges that the welfare of the Islamic community requires it.

15. The idea of a Messiah who will arise and purify the world is common to religions of the Middle East. Psychologically, the idea has provided the faithful with the hope in the establishment of a proper and just world in the future and made the pressures and injustices of everyday life more bearable. In Islam, the idea has appeared under the concept of the Mahdi (the Guided One). But Shiite Messianism is pessimistic concerning human beings and their capabilities, since they will fall away from the path of truth, faith, and justice and the End of Time will come when the copies of the Qur'an have become blank, the world full of tyranny and misleading clergy. Finally, there will be the appearance of the Hidden Imam. Khomeini, however, has personally caused the dissolution of such groups as Hujatiyyah, who have waited and kept vigil for the reappearance of the Mahdi and has instead urged the Muslims to take matters into their own hands.

16. Based on my studies in Iran in May–June 1988.

17. The Chinese government's ideological altercation with the Soviet Union in 1957–60, which resulted in the latter's withdrawal of its aid and technicians, provides another illustration of this phenomenon. This behavior cannot be fully understood without taking into account the legacies of the Opium War, the Boxer Rebellion, the mutilation of Chinese territory, Western imperialism, and the perception that even during the republican period the country's leaders were subservient to foreign powers (see Pye 1962, chap. 3; Pye 1972).

18. Reemergence of nationalist symbols occurred in the second half of 1988 in Friday prayers and sermons at the University of Tehran, in the mass media, and even in Khomeini's own pronouncements. It seems to reflect Iran's setbacks in the war. A parallel development occurred after the Iraqi invasion of Iran in 1980 but was halted and reversed by Khomeini as Iran's military position improved.

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COMMUNICATION, IDEOLOGY, AND DEMOCRATIC THEORY

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Using Habermas' theory of communicative action and his remarks on the legitimacy of the state under modern social conditions as a starting point, I combine normative democratic theory with the critique of ideology. I first outline four necessary-but-not-sufficient conditions of communication for democratic decision making: such agreements must (1) be formally and procedurally correct, (2) be cognitively adequate, (3) concern issues on which consensus or compromise can be reached, and (4) be free of ideology. The first three conditions form the core of a normative democratic theory, one that is not purely procedural, as many have argued it is. I then discuss the fourth condition and establish the relation between ideology and democracy. Taken together, these conditions not only provide an answer to troubling questions for democratic theory but also delineate the extent to which politics is rational and political claims are "truthlike."

Since the early modern period, the experience of democracy has been the source of both great expectations and bitter disappointments. Many Enlightenment thinkers had hoped to link democracy with moral and intellectual progress. These aspirations gave birth to strongly normative theories of democracy, most especially those of Kant and Rousseau, who saw democracy as the political and collective norm of human autonomy. But when these same norms were more thoroughly and consistently applied in the following century, it became clear that democracy alone could not fulfill all the promises of the Enlightenment. What proved inadequate were not the norms and ideals of autonomy but, instead, the practical and institutional bases by which these ideals could be realized. It is this problem that concerned Marx when he argued that political democracy did not prevent one group or class from acquiring enough power to gain control over decision making in so-called democratic political institutions; he also exposed the many discrepancies between the ideal rights of citizens and the living reality of modern human beings. As a centuries-old experiment, then, democracy has shown mixed results, leading some to reject it, others to declare it fundamentally irrational, and still others to minimize our hopes for it by defining it narrowly as mere legal and electoral procedures rather than as a normative ideal or a form of life.

It might seem difficult to put together these two strands of the legacy of democracy—the affirmation of its norms and the suspicion of its particular forms. Yet many philosophers in what may be called the "radical democratic" tradition have done so, including Marx, Mill, and Dewey. Their theories of democracy have typically favored participatory institutions. John Stuart Mill claimed that institutions remain democratic only through participation and that rights become real only if they are exercised. In this same tradition, John Dewey tried to show that insofar as democracy embodies a public procedure for testing and modifying norms and ends in light of common ex-

AMERICAN POLITICAL SCIENCE REVIEW
VOLUME 84 NO. 1 MARCH 1990

perience, it is as much a rational practice as scientific inquiry is. However, like Popper, both Mill and Dewey were at times led astray by an overly narrow concept of rationality modeled on science that could not directly serve as the basis of politics.

But it is precisely this expansion of the concept of rationality to fit the demands of a participatory politics that Jürgen Habermas develops in his concept of democracy as rational will formation, a concept that is normative and critical at the same time. First, he attempts to establish a "minimal cognitivist" ethics—a theory of the rational justification of norms—on the basis of which one can judge institutions and forms of life. The central claim of this normative theory is that political institutions and practical decision making can be governed by "discursive rationality" and truthlike, public claims to validity. This means that questions of justice, as much as questions of truth, can be settled "discursively," that is, in an institutionalized, controllable process of communication, through which members of a society arrive at rationally binding decisions about both the rules and goals of their common life.

This reinterpretation of democracy in terms of discursive communication gives rise to the second main element of Habermas' democratic theory: a revised theory of ideology, which becomes the critical self-reflective element of democratic justification. The theory of ideology can be recast so that it may now describe restrictions and barriers to social processes of communication, barriers that originate in structures of domination and that affect social processes of decision making and belief formation. These barriers function, in turn, to help maintain these same relations of power and domination, as well as the beliefs necessary to sustain them, even within democratic institutions.

I shall try here to show the fruitfulness of Habermas' new interpretation of democratic norms and their violation. I will also try to show that by relating these critical and normative aspects of democratic theory, a better set of necessary conditions for truly democratic decision making emerges, going well beyond the problems of proceduralism that plague Rousseau and some of Habermas' defenders. I systematically reconstruct Habermas' remarks on legitimation in modern society as outlining four conditions of communication that are necessary but not sufficient for genuine democratic decision making: such agreements must (1) be formally and procedurally correct, (2) be cognitively adequate, (3) concern issues on which either consensus or compromise is possible, and (4) be free of ideology. I first explore the first three conditions as the core of a normative theory of democracy, then discuss the fourth and the relation between ideology and democracy. In setting out these conditions, I shall show that it is possible to reconstruct Habermas' theory so as to avoid many of the problems that critics find in his views, namely, pure formalism and proceduralism, the exclusion of certain interests and claims in advance, the imposition of generalizable interests, and the impractical possibility of citizens' merely agreeing to disagree. If these problems are avoided, then the result is that the conditions of democratic political agreements cannot be judged purely formally but rather must be evaluated in terms of their communicative prerequisites and conditions; it also means that a democracy, no matter how well ordered, cannot entirely eliminate the possibility of communicative failures and must therefore develop ways to correct ideological communication. Thus, while I first deal with philosophical problems of democratic theory proper, I later treat institutional problems, including bureaucracy, expertise, education, complexity and participation.

The significance of Habermas' theory is

that it offers a systematic reconstruction of democratic theory in communicative terms. Like "empirical democratic theorists" such as Dahl and Neubauer, Habermas is concerned with the social conditions and prerequisites that make democracy possible—except that now those conditions concern the structure of communication and discourse. Second, like "economic theorists" of democracy such as Downs or Schumpeter, Habermas is concerned with the failures to achieve democracy—except that now these failures are not put in market terms but rather in terms of communication between participants. But unlike either of these two theories, Habermas' reconstruction aids and empowers, rather than undermines, agents' search for greater democracy and an adequate civic discourse (Ball 1988, chap. 6).

The Normative Conditions of Democratic Agreements

If Habermas were to develop an explicit theory of democracy, it would be cognitivist in its ethical dimension and participatory in form, having its origins in Mead, Rousseau, and Marx. Unfortunately, Habermas' discussion of democracy has not, as of yet, been systematic but remains scattered throughout his writings in other contexts. The closest Habermas comes to such a treatment is his first work, *Structural Transformation of the Public Sphere* (1961), where he analyzes the historical emergence of a normative idea of "public opinion" and its dependence on new forms of association like union halls, coffeehouses, salons, and clubs; the public spaces of modern cities; and new communications media such as newspapers, publishing houses, and even conversation. In his sociological writings, Habermas generally casts his discussion of democracy in terms of the concept of legitimation rather than in terms of a political theory. But it should nonetheless not be too hard to reconstruct the concept of democracy implied by his social theory, since the concept of legitimation for Habermas focuses on the relation of reason and social organization. Legitimacy is, after all, "the worthiness of a political order to be recognized" (Habermas 1978, 178) or, alternatively, the correctness of its claim to be right and just.

What does the legitimacy of a democratic order entail? Legitimacy refers to the convincing character of reasons that justify political decisions or, more generally, validate practices and institutions. Indeed, it is true for any meaningful social action that agents are able to give an account of what they are doing by appealing to reason, if only to a stock of traditional or commonsense knowledge. Historically, the state emerged as an institutional solution to a number of problems of complexity, such as geographical separation, social conflict, and the organization of activities; this type of organization required new legitimating reasons, which were supplied by expanding mythical narratives into cosmologies and, finally, ontologies. The advance in social organization required a corresponding advance in cognitive abilities and the acceptance of new types of convincing reasons. However, the ability to give reasons can always be taken one reflective step further: we can also discuss the reasons themselves, in what Habermas calls a "discourse." Such reasons attach to speech acts, in their claims to be accepted by hearers as valid. Habermas generalized Frege's idea of truth conditions to include three types of "validity claims": utterances may be true, normatively justified, or the truthful expression of intentions and desires (Habermas 1984, 276). Such claims can themselves be made the subject of communication in various types of discourses—theoretical, aesthetic, and practical—each with its own claims and procedures.

Self-reflective communication in discourses typically involves public argumentation of various sorts. Arguments are not simply to be interpreted logically as inferences but also pragmatically as self-reflective, reason-giving activities, whose social function is to give hearers "rational motivation" to accept a claim as valid. So accepted, the claim then acquires "intersubjectively binding force," in that actors may appeal to it in creating agreements, in coordinating plans of action, in repairing intersubjective bonds, or any of the other myriad uses of language. Thus, in order to facilitate these integrative and reproductive functions, discourses and patterns of justification become institutionalized in various practices, such as jurisprudence, art criticism, science, and democracy. Indeed, democracy has a special self-referential quality as a form of practical discourse; in it, as Habermas puts it, "the formal properties of justification themselves obtain legitimating force" (Habermas 1978, 84). This means that in democratic institutions, discursive rationality itself becomes the normative basis of its now-reflective claim to legitimacy. Once this level of justification is achieved, previous nondemocratic legitimating grounds are no longer convincing or binding and hence become incapable of establishing reflective patterns of interaction or recognized uses of political power.

This shift away from previously acceptable, nondiscursive reasons has enormous consequences, some of which Rousseau elucidated in his conception of the moral transformation of the self in the *Social Contract.* Democratic institutions require a new type of moral and social identity, which Habermas affirms as "the rational content of modernity." For Habermas, as for Durkheim, society is a symbolically structured, moral reality such that each of its members internalizes this structure as part of their acquisition of identity. In modern societies, members can only internalize these structures reflectively, more frequently as reasons and less often as causes, as they affirm the rational basis of practices and institutions. Thus, the shared, identical set of beliefs and attitudes, those that make up what Durkheim called the "collective conscience" of traditional societies, contrast quite sharply with the modern "general" or "rational" will; the latter is collectively shared yet not identical for each, a unity with many voices. George Herbert Mead described this new, more fluid identity quite well as follows: "A highly developed society is one in which individual members are interrelated in a multiplicity of different intricate and interrelated ways" (Mead 1934, 268). This very individuation is both an achievement and a problem on cognitive, moral, and emotional levels; it is constantly threatened by a loss of the cognitive perspicuity, the moral identity and the emotional identification that are necessary to sustain a common life under modern conditions.

How is this problem of individual and collective identity to be solved on this side of modernity? Democracy is one solution inasmuch as it does not presuppose that shared beliefs and common interests already exist. Rather, it is a *process* by which a "general" and nonidentical collective will is generated and formed. Democracy is therefore a practical hypothesis about how convincing reasons and collective identities may emerge out of a public process of deliberation. As Habermas puts it, democracy "ties the development of social systems to control through a politically effective institutionalization of discourse" (Habermas 1973, 393). Its proper goal is not simply to maintain individual freedom or the peace and security necessary for commodious living but to establish intersubjective agreements free from domination, violence, and simple tradition. For this to occur, the topic under discussion must itself be that of how to arrive at a conscious, rational decision about the structure and future of society.

If democracy is such a progress of forming a new type of collective will, it is necessary to specify the conditions under which this will may be "rational" and how the interests which emerge may be "general." This effort makes up the core of Habermas' theory of legitimacy. However, as he has articulated it so far, Habermas' conception is rather limited. He often seems to indicate that certain formal—that is, merely procedural—conditions of communication are sufficient to guarantee the rationality of the will and consensus formed in a practical discourse. For example, Habermas asserts something like this in his 1975 postscript to *Knowledge and Human Interests:* "The discursively formed will can be called 'rational,' because the formal properties of discourse and the deliberative situation sufficiently guarantee that a consensus will arise through appropriately interpreted, generalizable interests" (Habermas 1975, 100). Habermas further specifies these formal properties in his "ideal speech situation," perhaps the best known but least understood aspect of his theory of practical discourse. This counterfactual construction presumes to permit us to judge how the properties of actual discourse and deliberative situations might fail to issue in genuine, well-considered agreements. Hence, its primary purpose is purely procedural, in that it sets out constitutive rules of successful practical discourse for consensus formation, such as that all speakers must have an equal chance to speak, raise objections, or employ arguments. Is this construction a sufficient basis for a democratic theory?

Most of Habermas' critics and defenders alike stop here, seeing his ethics of discourse as yet another, perhaps better, version of what Rawls calls a "pure proceduralist theory" (Baynes n.d.). Indeed, there are strengths to a strictly proceduralist position. As Richard Bernstein points out, it is easier to defend such a modest and weak claim for the theory of communicative action. It is also more relevant to political decision making in democracies, since the requirement of autonomy means that participants do not lay out a blueprint for the good life in advance. The same is true of modern communicative rationality in general. As Bernstein puts it, "Such a communicative reason is only conceptualized as a procedural rationality; it is not sufficient to judge or dictate a substantive form of life" (Bernstein 1983, 191). But even such a weakened claim does not escape basic philosophical difficulties common to any purely procedural theory of rationality. First of all, there are doubts about the completeness of such a theory as a basis for democracy: procedural requirements cannot define their own conditions of adequacy. For example, the condition that all participants get a chance to speak could be institutionalized in a rule about turn taking. Yet even with this rule, some turns or chances to participate may be inadequate, as they are performed without the proper capacities or made in a situation in which they cannot be effective. Rules and procedures, too, must be interpreted and applied appropriately; and both interpretation and application require their own criteria of cognitive adequacy not specified by formal requirements alone.

A second set of objections to pure proceduralist democracy concerns the claim that procedures can be the basis or foundation of justification. Procedural justification is not only too weak to support democratic institutions, it is also paradoxical in two senses. First, justificatory appeals to procedures are paradoxical insofar as they are self-reflexive: the rules and procedures of public practical discourse can only themselves be justified through a public practical discourse. However, this paradox of self-reflexivity does not make procedural justification self-defeating, only circular, and hence never self-sufficient. Against Bernstein, I would argue that the turn to communication and discourse alone cannot save a procedural theory of rationality or democracy, so long as it is a "pure" proceduralism. Second, it is obvious that the same paradoxes exist for formal criteria of communication as for formal criteria of voting, such as majority rule: just as majority decisions may be undemocratic, so, too, all the formal conditions of communication could be satisfied and the outcome of the collective discussion could still be a nongeneralizable interest. Here, again, this objection does not mean that procedural rules are irrelevant; indeed, they still make up the core of any discursive theory of democracy, but only as one of several necessary conditions of communication. To give them any stronger justificatory weight would eliminate the need for discourse itself. If procedural and formal conditions were all that were required for a democratic consensus, then dialogue and discussion would not be needed. Like the Kantian agent, a single speaker could anticipate the results of such a constrained discourse. Procedural correctness can therefore never have the same role in democracy that truth has in science. Unlike science, which is a form of public justification based on grounds that do not have to do directly with the discussion itself and how it is conducted, politics is doubly self-reflective. As Benjamin Barber points out, it is itself a political, not a theoretical, question as to what politics even is (Barber 1984, 154). This double reflexivity does not, however, exclude the possibility that the conditions of discourse and communication can take on a normative role. Rather, if politics is not to become arbitrary, these conditions must regulate, and cannot determine, the formation of beliefs and desires in political discourse.

If we look more closely at Habermas' own description of legitimacy, he clearly indicates that he is not a "pure" proceduralist when he uses the phrase *appropriately interpreted, generalizable interests.* What makes an interpretation "appropriate" must be that it fulfills certain cognitive, as well as formal, conditions. It is also important that it be an explicit interpretation of an interest or a need or motive. Such conditions, however, should not be too strong or specific, as is often the case in pure proceduralist and "liberal" theories of democracy. Proceduralist theories typically use various devices, ranging from the "veil of ignorance" to impartiality, in order to constrain the types of needs and desires that can be expressed in democratic discourse. It may be possible to interpret Habermas' phrase in the same way or to construe the ideal speech situation as just such a device, and some critics have done so (Walzer 1987, 10). The problem is once again that such proceduralist constraints on content would restrict the scope of democracy and decide in advance what the process of public discussion will be and the type of need or desire that may be decided upon. This is exactly the model of liberal democracy that Bruce Ackerman develops as "constrained conversation" under the limits of neutrality. Neutrality, Ackerman argues, is necessary because political talk is about power. At the same time, however, he admits that "the breadth of exclusion it requires is quite obvious" (Ackerman 1980, 10–11). Understood discursively, neutrality is too strong a constraint on self-expression in democratic discourse. It would reduce the scope of political conversation to empty formal rules or to issues about which there is already widespread agreement if not unanimity.

Against "liberal" theories, Habermas gives a broader account of cognitive content. No interest, no need, and no desire is excluded in advance so long as participants are willing to subject them to what he calls "post-conventional moral justification" and public reinterpretation. Rather than using norms of discourse as exclusionary principles, as liberals like

Rawls or Ackerman do, Habermas' cognitive, postconventional conditions of democratic discourse serve only to specify the class of reasons and motives that can form the basis of a process of reaching rational agreements.

Part of giving a postconventional justification requires that these interests and needs must be formed post factum, through the process of communication itself. Otherwise, the proceduralist falls into the dilemma Walzer constructs to criticize Habermas: either the discussion is so limited as to make dialogue superfluous or—if one may say anything at all—the results may turn out to be quite undemocratic and even immoral (Walzer 1987, 11). Habermas escapes the first horn of the dilemma since he neither employs a pure proceduralist principle like Rawls nor employs an exclusionary principle like Ackerman. But the noncognitivist, relativistic theory of democracy cannot escape the other horn, as is the case with Benjamin Barber's conception of "strong democratic talk." For Barber, "strong democracy" is supposed to transform conflict and disagreements into cooperation and agreement. But this is to be done under the basic epistemic condition of politics: "the absence of an independent ground" (Barber 1984, 129). The problem with Barber's proposal is that this absence threatens democratic practice with arbitrariness, since it has no epistemic basis. Without criteria or norms, any de facto resolution of conflict or emerging agreement would count as "democratic," no matter how distorted or ideological. Habermas escapes this problem by appealing to cognitive standards of postconventional interpretation and justification: the requirement of subjecting one's own needs and desires to postconventional interpretation means that only the needs and desires that can be justified publicly may be adopted. This cognitive requirement admits a wide range of needs and desires and does not exclude even self-interest in advance. When Geuss says that Habermas so constrains communication that it may only deal with those opinions already destined to converge, he misses the point. Prior to the discourse there is no "fact of the matter." As for democratic discussion's permitting strange and immoral results, this objection misconstrues the political purpose of a normative theory of democracy, which no more assures that actual democratic practice yields just results than a theory of truth will guarantee that all of our statements will be true. Thus, the alternative to the two horns of Walzer's dilemma (proceduralism or anything goes) is a normative theory of democratic discourse serving to articulate a regulative ideal for communication in collective decision making and not excluding or determining any need or desire in advance.

Postconventional, cognitive conditions not only resolve the dilemmas and problems of procedural paradoxes but also the intractable difficulties of trying to order fixed, incommensurate preference rankings, something Arrow showed to be impossible. Problems of ranking diverse sets of preferences and conceptions of the good can be solved only if participation in democratic discourse is an active process of forming one's will and judgments in relation to others. As Habermas puts it, distinguishing his view explicitly from Rousseau but also implicitly from Rawls, "Discursively redeemable norms or generalizable interests have a non-conventional core; they are neither empirically given nor simply posited; rather they are in a non-contingent way, formed and discovered" (Habermas 1975, 175). Thus, the problem of democracy is not how to find some optimal solution or standard rule for ranking incommensurate values. Rather, democracy is the political means of forming a postconventional identity where everyone affected by a decision must be able to participate in the agreement. In so doing, participants change their beliefs and desires and form their will, see the validity of the implied norms, and thereby begin to transform their needs and desires in light of them.

Mansbridge's case studies of participatory democratic institutions reveal just such a process in free and open decision making (Mansbridge 1983, 163–82). Here Ackerman's constraint on participants in neutral dialogue—namely, that they may not assert that their conception of the good is superior to that of their fellow citizens—is too weak: it would minimize revision to such an extent that under such cognitive conditions, changes in beliefs and desires through practical discourse of the sort Mansbridge reports would be impossible. Barber's "strong democratic talk" is also too weak, since participants are left with no clear epistemological basis for public attempts to convince others of the correctness of judgments about the various possibilities for cooperation and agreement.

But even the addition of cognitive conditions is not enough. There are at least two further conditions for democratic will formation that are only hinted at in the phrase *appropriately interpreted interests* but that are nonetheless implicit in Habermas' normative view of communication. First, the type of issue under consideration needs to be specified: Is it one capable of consensus or one for which compromise is the only possibility? Second—and perhaps this is a subcondition under cognitive aspects of democracy—the communication in democratic deliberation must not be ideologically distorted. Both of these conditions must be spelled out further if a fully communicative interpretation of participatory democracy is to deal with all the aspects of the historical experience of the limits to democracy.

Even with his formal and essentially individualistic concepts, Rousseau already recognized that not everything can be decided by the general will. For Habermas, the point is not—as it was for Rousseau—to separate the general will from the will of all but to decide which issues can and cannot be settled in a consensual manner. There are certain limitations on practical discourse that are not present in theoretical discourse, since participants in it are involved actors, not just reflective speakers. Failing consensus, however, participants in democratic communication can still seek compromises; and their production may be the purpose of many discursive, participatory institutions. Even then, the course of public interaction is not to be left to strategy and conflict. Consensus is not the only form of democratic agreement. But because of the constraints on strategy and conflict, it does seem that consensus and compromise exhaust the possible forms of democratic agreement. In democracy, the issue at hand must be decided in such a way that a general will is formed. In theoretical discourses, other forms of agreement are possible short of a general will. As is often the case in science, problems and claims may remain suspended. However, in a practical discourse, if we agree only to disagree, no common will is formed, problems and conflicts remain unresolved, and political discourse fails.

Institutions oriented to compromise can also be designed discursively. Compromises may be formed through mediation and negotiation, not just by balancing power or trading off unresolved private interests (Dryzek 1987). Compromises, too, can be democratic and undemocratic, rational and irrational, arrived at by force or by argument. Their communicative conditions are just as rigorous, having to do with the creation of conditions of equal power, especially in relation to agenda setting and turn taking. First, the issue at hand must be concerned with interests and needs that are genuinely nonconsensual. Second, there must be an institutionally secured effectiveness of agreements, so that the process of reaching a compromise itself is not only formally legitimate but actually carried out. Third, self-interest alone would not supply a secure-enough cognitive basis for the sort of sustained communicative interaction that makes up the setting for most acts of bargaining and negotiating. It is obvious that the appeal to the necessity of compromise does not remove either the decision or the decision-making process from democratic criteria. Nor should compromise be seen as final, in that the outcomes could unintentionally endanger the possibility of further consensus formation. In a democracy, agreement seems to be more desirable than compromise and bargaining, at least as a goal or a regulative ideal, although a variety of discursive institutions for conflict resolution, mediation, and negotiation are necessary for problem solving and the formation of acceptable agreements.

If these constraints exhausted the conditions for democratic agreements, it would still not be entirely possible to escape problems of the tyranny of majorities and of empty formalism; nor could we make sense out of Habermas' own criticism of merely "formal" democracy in *Legitimation Crisis* (1973, 36–37). The fourth and final condition for democratic communication is that it be free of ideology, that the process of deliberation be "unconstrained" by restrictions and causal influences of which participants in democratic institutions may not be fully aware. In opposition to Daniel Bell, Habermas believes that democracy does not rule out ideology any more than science does; in fact, like science, democracy creates new possibilities for ideology, as Marx pointed out. Traditional theories of ideology have focused on truth content, defining ideology in terms of actors' false beliefs about their society. Such theories require strong causal explanations that are often so encompassing as to involve the theory in a self-referential contradiction (as in the case of the reduction of cognitive claims to interests). But what is really explanatory in such theories is not so much what they show about the content of such a belief as how the belief affects social relationships in society. Despite the institution of various reflective forms of discourse, the social process of communication within actual institutions may still be affected by relations of power and domination in society. Such influences include barriers and restrictions connected with gender, class, social structure, and relations of power, all of which may undermine the conditions for the success of such public practices of justification and agreement. The potential for ideology in such practices is seen in the fact that both scientific theories and democratic decisions may be fully correct in that they pass procedural tests of verification or the actual agreement of all those involved—and yet be ideological because those tests and agreements were conducted under conditions that restricted communication in the relevant scientific and political discourse communities. If this is true, reflective participants must make use of social-scientific explanatory theories to create tests for "distorted communication," that is, explanations that test for restrictions and barriers affecting decisions and outcomes and limiting the full participation of all. One role that the critique of ideology can play in a normative theory of democracy is to explain why democracy is as limited as it is in contemporary society, despite the cognitive achievements of postconventional attitudes and the institutional achievements of universal suffrage and the rule of law.

Ideology and Democracy

In the classical Marxist tradition, the discussion of ideology and democracy has been limited by the quite-narrow scope of the definitions of both concepts. In Marxism, as in much of recent liberal political theory, *democracy* is usually identified with Western, liberal, capitalist institutions. *Ideology,* too, is often conceived of in terms of a specific, historical form of ideology, the religious-metaphysical worldviews that lost their social integrative force in early modernity. It is the mirror image of this conceptual map of Marxism that allows a theorist like Daniel Bell to speak of "the end of ideology" in democracy and science even while both practices continue to have great ideological potential.

This potential is found in the fact that appeals to the results of both practices may undertake the same ideological function of justifying social practices that appeals to religion and metaphysics formerly filled. For example, under present conditions of an administrative state, claims about scientific rationality may become justifications for certain political arrangements of power relations, as in the case of technocratic interventions. The social use of scientific truth may bring about a confusion between theoretical and practical discourses and their forms of validity. Ideological functions can be taken over by appeals to democracy itself, as when a result is justified simply by the fact that it followed a certain rule. Similarly, within existing democratic institutions, restrictions on communication may occur despite the existence of proper procedures. Take, for example, the treatment of interests within a process of democratic decision making. Often a consensus may be reached on the presupposition of a widely shared, common interest that does not exist. Such a consensus may be a false one and merely encode relations of power unthematized by the discourse. Conversely, in some institutional settings, the rules, procedures and styles of discussion may simply presuppose that interests always conflict and thus inhibit the formation of strong forms of agreement. It may also be the case that due to social circumstances of restricted information or expressive capacity, some participants do not yet know their own interests and needs. In all these cases, such democracies restrict and distort communication within their institutions, despite the achievement of some form of public, reflective justification. Of course, the absence of distortions does not guarantee the truth or correctness of the results of such practices, which must undergo continuous self-testing and revision. In general the absence of ideology is, however, a necessary-but-not-sufficient condition for the validity of the results of a discursive practice. Unlike the case of science, the self-reflectivity of any practical discourse makes it impossible to be right for the wrong reasons.

If Bell and others are wrong, what, then, is the relation between *ideology* and *democracy?* Here, too, Habermas' own writings are not entirely helpful since, for various reasons, he has failed to develop a theory of ideology as distorted communication. Seen in the framework of Habermas' theory of communicative action, it is clear that they should stand as contrast classes. For Habermas, both *ideology* and *democracy* refer to processes and structures of communication within modern societies. According to the normative theory developed in the preceding section, *democracy* refers to an institutionalized process of communication in a society, through which members publicly deliberate and arrive at binding collective decisions. *Ideology,* by contrast, restricts or limits social processes of communication by structures of domination or power. Such distortions certainly can affect both the conditions under which discourses take place and the processes of communication within them. The theory of ideology therefore analyzes the ways in which linguistic-symbolic meanings are used to encode, produce, and reproduce relations of power and domination, most significantly within institutional spheres of communication and interaction.

Typically, socially distorted communication is also "systematic," that is, participants may not be aware of the restrictions in communication in their societies (Habermas 1970, 205–18). This is particularly apparent in democratic institutions that are supposed to be free and open. It is precisely within these institutions, however, that restrictions in communication undermine the very possibility of collective self-determination. The theory of ideology, then, must analyze the variety of dimensions in which communication can be systematically restricted, down to the details of interaction within the institution—such as who participates and who counts, how they participate and deliberate, and why certain issues may or may not be discussed or decided upon. Any limits not strictly related to conditions for reasoned public discussion distort the process of communication and can skew the decisions reached by such discussion. Such barriers to rational agreement indicate the presence of ideology in a democracy. The critique of ideology, therefore, does not deny the positive achievements of democracies in institutionalizing forms of communication related to practical discourse; but it strives to push them in the direction of even more democracy. It could be argued that the critique of ideology is an integral part of democratic communication itself, so that its institutional form may be corrected according to insights into its own limitations. Without such self-critical reflection, democracies could not learn; and practical discourses could not reflect on the communitywide biases that occur in the structures and conditions of institutionalized communication. Self-correction permits greater democracy and problem solving as self-correction in the sciences permits greater truth and efficiency.

If ideology and democracy can be related in this way, it would be fruitful to develop a systematic set of all the possible ideological distortions and restrictions on practical discourses. Generally, Habermas has had a limited model of distorted communication, as speech involving an unavowed strategic intention. As he puts it, all distortion in communication "can be seen as the result of the confusion of action oriented to understanding and action oriented to success, of strategic and communicative action" (1984, 373). Such a limited concept would not very easily fulfill the empirical purpose of uncovering the full range of possible restrictions in communication in practical discourses. A speaker's intention will not bear this descriptive weight, even in the case of self-expression. The nonavowability of an intention makes it particularly unsuitable even for describing a speaker's own unconscious limits on self-expression, since they do not always involve such confusions in any obvious way. The *inability* to express a need or desire can distort communication as much as unavowed strategic intentions. Here the formal apparatus of the theory of communication can be put to a useful empirical purpose and save the theory from Habermas' limited description.

According to most theories of communication from Bühler to Jakobson to Habermas, communication occurs in various types of acts, each reflecting a different social function of the use of symbolic media like language. Symbolic media may be used to constitute social interaction, to express intentions and desires, or to represent states of affairs in the world. While the theoretical underpinnings of such distinctions in pragmatics are still essentially disputed, there is, nonetheless, widespread agreement that a descriptively adequate theory must account for at least these three functions of communication: interaction, expression, and representation. Using these descriptive distinctions as our heuristic guide, we can develop a systematic account of ideology as distorted communication in each of these dimensions (Bohman 1986). I will confine myself here to discussing a few such distortions in the practical discourse that is democracy and show how recent social-scientific criticisms of democratic institutions can be accounted for in this general framework.

If democracy is defined as an institutionalized form of communication through which actors determine the means, ends, and rules of their association, the first place to look for distortions is in the most direct effects of power and domination on interactions leading to decision making. Much of classical democratic theory, particularly that of Rousseau, is concerned with the conditions under which this interaction can issue in a "general will" in collective self-determination. These conditions can be translated into communication-theoretic terms. In general, ideological barriers on practical discourse restrict acts of self-expression and mutual understanding by undermining the conditions of their success; they do this by using consensual means to establish, justify, and reproduce relations of domination in a manner consistent with democratic procedures. Ideology therefore restricts communication and interaction in order to produce the effect of consensus where none can be produced discursively except by force or self-deception.

Let me further specify this abstract characterization by laying out conditions of communication in democracies more precisely. Often, procedural and formal aspects of the institutional setting of communication are in further need of democratization. For example, certain institutional arrangements make effective participation of those affected by a decision impossible, not merely practically but because the institution fails to create a framework in which all have an equal chance to be heard and to affect the decision. This reduction of the scope of democracy can often be done structurally, rather than by mere force.

One way to limit such scope is to remove key decisions from the democratic process, often at the agenda-setting level before discourse has ever begun. Baratz and Bachrach have pointed out how bureaucratic organizations within democratic polities undermine conditions of democracy by filtering issues out of the decision making process. This "mobilization of bias" results in a nondecision, that is, in the successful exclusion of a whole range of possibilities that might otherwise be rational but challenge the existing social structures and relations of power (Bachrach and Baratz 1970, 39–51). If an issue or demand is prevented from reaching the public arena for discussion, there has been a successful nondecision. Nondecisions function to distort communication by disallowing legitimate conflict and creating a pseudoconsensus. All this may be done in terms of the existing resources of the political-legal systems—through norms, rules, and principles, as well as through the use of noneffective participation like public hearings after the fact (Edelman 1967, 57). Bachrach and Baratz go so far as to assert that most public policy decisions involve nondecisions. For example, Bachrach and Bergman have analyzed the bias of population control policies toward technological interventions in the fertility rate and found them to be hardly the most efficient way of lowering population growth (1973, 7–8). Rather, the prevailing policy is a strategy more consistent with bureaucratic intervention and with the more primary goal of preserving existing relations of power than of redistributing wealth or life opportunities. Thus, not only do nondecisions violate formal conditions of democratic participation, they also reveal serious ideological biases on the cognitive level, leading to the repeated failure of nondemocratic institutions to solve basic social problems and to understand and distribute information. It may well be that nondecisions are the focus of conflicts between bureaucratic and democratic organizations. Social movements often try to turn nondecisions into decisions by using the public sphere in a noninstitutional way. In such cases political institutions need to be expanded by extrainstitutional means, since the institutions themselves reveal consistent structural biases. But the role of ideology critique is always to show that some mechanism of reflective, direct public deliberation can take the place of undetected structural limits and thereby show that democracy can be increased. A general form of democratic change is the reopening of decisions made in closed institutional discourses by using the resources of the more-open, public sphere of communication. This procedure may even increase the problem-solving capacities of political institutions (Dryzek 1987). It may also be true that once a consensus is reached, the delegation of its execution to experts and administrators may be effective, subject to the continued existence of an already-established and widely shared generalizable interest.

While nondecisions provide a good example of criticisms internal to the process of communication and deliberation itself, external and material conditions can also influence and restrict communication and consensus formation. Rousseau remarked long ago that enormous inequalities of wealth and status make the "general will" impossible. Here, too, the same is true of genuine consensus. If participants enter into the practical discourse of democracy with large-scale inequalities, the difficulties of reaching consensus and the necessity for ideology increase in proportion to the degree of inequality. As many critics have pointed out, capitalism and patriarchy are inconsistent with democracy in that both organize society in radically inegalitarian ways, down to structures and patterns of communication. Robert Dahl's recent criticisms of economic inequalities show how they create political inequalities among citizens (Dahl 1985, 54–55). It is important to note that inequalities themselves are not ideological but that they produce ideological restrictions and distortions in processes of communication and their outcomes in a variety of ways. As the classical critique of ideology shows, they tend to instrumentalize political institutions by making them take on extrapolitical, economic functions, the policies of which consistently serve the purpose and long-term interests of specific, privileged groups.

Even more directly, inequalities affect the abilities and competences with which people enter public discussion and deliberation. Basil Bernstein's writings on class differences related to speaking offer a good example of how public discourse typically requires the "elaborated" codes of privileged groups (Bernstein 1967). Such differences in codes may become the basis for asymmetries in effective expression and the exclusion of some from full and adequate participation in a public discourse. By constituting the range of discourse available to speakers, "codes" are the result of the way social relationships act selectively to delimit what is said and who says it. To show how differences in codes reflect and maintain social structures, Bernstein examined the different modes of expression of lower and upper classes. He discovered that the language use of the upper classes tended to be less indexical and more universal, as well as more lexically and grammatically elaborate. Whether or not this particular example is empirically correct, it is easy to see how such a difference could give rise to differences in effective participation in practical discourses and to structural assumptions about how one takes on the role of an expressive speaker. Hence, codes may embody noninstitutional, prediscursive barriers to self-expression for those who differ in competence, styles of expression, vocabulary, and even, in the case of multilingual societies, the very language spoken.

Under these conditions of communication, a purely procedural, "liberal" notion of democracy, based on majority rule, ends up privileging members of genders, strata, and classes who "know what they want and how to get it" (Offe 1985, 295). The acquisition and exercise of certain politically significant competences and abilities surely requires prior access to social goods. The lack of any effective means of participation and self-expression has a stabilizing effect on relations of domination in democracies, since the only way to develop higher-order communicative and social abilities is already to participate in public discourse. Distorted communication in democracies puts the powerless in a double bind. Democratic justice requires not just procedurally correct communication but also egalitarian standards with regard to the conditions and social goods necessary for the development and exercise of expressive social competences. Public education is only the beginning, since it may only reproduce these same inequalities of competence and expression.

In democratic societies, reproducible patterns of domination require that the dominated group remain inarticulate, that its needs rarely and ineffectively become themes of public discourse. For the sake of some equality of self-expression, a certain degree of both material welfare and commonly possessed social goods is necessary if every citizen is to develop communicative and expressive abilities. Poverty by itself is exclusionary in a democratic society, since it not only inhibits the development of complex social and cognitive abilities but excludes people from information, opportunities, and experiences shared by many. Educational institutions should thus be more clearly oriented to democratic goals of effective citizenship, like the development of capacities of public self-expression and shared understanding. Teaching autonomous and authentic self-expression is a difficult task under any conditions; and it is a potential setting for further distorted communication, as vanguard theories of political change have long demonstrated. Indeed, such education is a political act that, if it is not itself to become a form of domination by the "educators," must be undertaken under the same conditions of will formation as democracy in general.

Here Habermas' emphasis on new social movements can be placed within a broad understanding of the "educative" effects of participation, something emphasized by many within the radical democratic tradition. Such effects include not only the development of abilities related to self-organization but also the acquiring of a wide range of discursively formed interests, ideals, and norms. Many of the social movements today are "citizens' initiatives," insofar as they emerge as subinstitutional associations based on shared interests and ideals, often with the political goal of demanding greater participation of citizens in the decision-making processes of the state. A democratic society should foster the conditions necessary for the formation of such groups, since it is only in the context of such associations that citizens develop the skills and experiences necessary for democratic will formation. Such organizations also provide a much-needed space for experimentation and innovation, for what Habermas calls "social learning." Opposition to such groups is typically greatest from the nondemocratically organized institutions they oppose, such as bureaucratic and capitalist enterprises. Except when sanctioned by generalizable interests, such institutions are the main barrier to the continual expansion of democratic institutional structures through acts of democratic communication within the subinstitutional public sphere.

Finally, there are also ideological distortions of democratic communication due to false beliefs about the nature of democracy itself. These beliefs are ideological not only in being false but also in functioning so as to restrict communication in democratic institutions. Given the enormous inequalities in contemporary Western societies, it is not surprising that beliefs about democracy in them tend to be highly ideological. Indeed, the predominant interpretation of democracy both in popular culture and in much "political science" is noncognitivist and economic rather than rational and political. William Riker, for example, argues that participatory or "populist" conceptions of democracy are incoherent. Based on rational choice treatments of the problems of collective action and decision, Riker shows that no procedure or rule for aggregating preferences could arrive at a consistent or nonarbitrary set of outcomes or decisions for all distributions of preferences (Riker 1982, 36). If this is true, no popular will can ever be found, since for any such procedure there exist demonstrable paradoxes and inconsistencies. The result is that practical questions, such as moral and political decisions, are not rationally decidable or subject to reflective tests. On this view, democracy is not a discursive practice of public deliberation but rather is concerned with institutional ways of aggregating fundamentally unchangeable and nonrational preferences. Democracy is seen in the model of voting rather than discussing or participating in a collective process; Arrow's theorem seems even to lend mathematical support to the impossibility of further democratization, since there can be no optimal or even consistent ordering of random, given, preference rankings.

While I do not in any way dispute the validity of Arrow's insight into the possibilities of preference aggregation, I question Riker's interpretation of these results as demonstrating the impossibility of collective, democratic decisions. All that Riker has shown is the incoherence of a proceduralist theory of democracy combined with emotivism about norms and values. To be coherent, a conception of democracy as collective will formation must be cognitivist, that is, one oriented to normative standards of correctness and not to the simple belief that "the opinion of the people must be right" (Riker 1982, 14). Rather, needs and preferences are not per se valid or invalid but must be formed through the process of public discussion and interpretation of them. The initial opinion or preference of the majority—or even of all—may turn out to be incorrect. Democratic discussion ought to be guided by ideal preferences and ideal consensus, which makes any given agreement, even if unanimous, subject to the criticism of a better and more adequate interpretation and judgment should one emerge out of further public debate and deliberation. A cognitivist, normative theory of participatory democracy would not be incoherent in Riker's model, even though the outcomes of different discussions may vary according to differences in information, self-knowledge, and competence of participants. The ideological character of Riker's arguments becomes clear in that they provide the basis of limiting the scope of democratic institutions. Since democracy is a self-reflective practice, beliefs about the practice itself have a constitutive role and affect how it is carried out. Hence, the self-interpretation of participants is yet another example of the ideological potential of democracy, no matter how well structured its institutions or how well formulated its procedures. The truth of "civic republican" views of political institutions is their recognition of just this active role of citizens and their self-interpretations.

The problem with such noncognitivist interpretations of democracy is not just their theoretical one-sidedness. Rather, such beliefs become ideological by how they are used to justify restrictions on democratic participation, particularly the establishment of leadership elites. Riker uses rational choice theories reductionistically in this way to justify voting as a veto to leadership elites rather than as one way to express judgments about the course of common public life. Because democratic institutions and leadership cannot be rationally justified, they are instead invested with a civil-religious character. Loyalty is based on some nonrational identification like nationalism or, failing that, self-interest, now that legitimacy is no longer conceivable in terms of the intrinsic character of the institutions themselves. On this model, political decision making is confined to voting, as a veto to nondemocratic decisions and as a means of engendering mass loyalty—while actual decisions are made by technocratic elites (Schumpeter 1950; Riker 1982, 243). Politics itself becomes purely symbolic ritual or spectacle (Edelman 1988, 111). While both require participation, no longer is an effective process of practical decision making subjected to reflective, rational control. Under these conditions, democracy is pure ideology, unrelated to the practical course of social life and the distribution of power.

Conclusion

This reconstruction of a normative theory of democracy, linked with a theory of ideology, has shown the political significance of some of Habermas' recent work, as well as a way out of some troubling problems of proceduralism and political rationality. Such a systematic reinterpretation goes well beyond the theories of Kant and Rousseau in specifying a basic set of conditions for valid democratic agreements. The emphasis on communication provides a fruitful way for democratic theory to incorporate the critique of ideology, now construed as a means of self-reflection and self-correction of communication in political institutions. As a whole, Habermas' description of communication implies a conception of the public sphere as an ever-expanding space for social learning, criticism, and autonomy—all of which may now be defined collectively as well as individually. While its particular forms may vary according to social and historical conditions, democracy in this sense is a necessary political component of a free social order.

Even granting the limitations of historical circumstances, the conception of democracy argued for using a theory of communication must ultimately be radically participatory. For Habermas, democracy marks one of the basic thresholds of social evolution and is at least one of the necessary conditions of human emancipation. It is still an open question whether these strong conditions for communication in practical discourses can be adequately institutionalized. Some have argued that the demand for participation is inherently impractical, that the demand for more democracy leads to social instability. Under present circumstances, participation is destabilizing, in that it is inconsistent with the unequal distribution of power that now exists. One can, however, easily imagine a future society in which participation and communication are self-sustaining. If adequate participation is made the central criterion of legitimacy, it will have the opposite effect in the future to that which it has today: it will further its own empirical conditions of possibility by cultivating the very rational attitudes and critical abilities necessary for it to exist. The more a society is based on participation, the better its members are able to rule themselves. Problems of complexity and numbers, not human irrationality, are the strongest practical objection to pursuing radical democracy under contemporary conditions. Habermas himself takes these problems very seriously, often to the point of almost Weberian political pessimism in recent political essays. However, complexity may be an even greater limit to *non*democratic institutions, which lack the resources of competent and motivated citizens. Whatever the real practical limits complexity places on the process of democratization, they cannot be set in advance but can only be discovered experimentally through social learning in democratic institutions themselves. Until these limits are reached, the goal of a democratic theory is *more democracy.* Such expansion is possible in modern democracy so long as citizens find in the public sphere a discursive space for criticism, learning, and new forms of association that can test the limits of their political institutions and beliefs.

Note

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MANDATES AND POLICY OUTPUTS: U.S. PARTY PLATFORMS AND FEDERAL EXPENDITURES

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Political parties in the United States are usually regarded as too weak and decentralized, too much the prey of office-seeking politicians and special interests, to function effectively as programmatic, policy-effecting agents within the separation of powers. This has been taken as a serious flaw in the U.S. version of representative democracy, prompting cycles of proposed reform; criticisms of the existing set-up as a capitalistic sham; or alternative justifications of the system as pluralist *rather than strictly* party *democracy. Our research challenges these assumptions by demonstrating the existence of strong links between postwar (1948–1985) election platforms and governmental outputs. Platforms' sentences, coded into one of 54 subject categories, are used as indicators of programmatic emphases and are related to corresponding federal expenditure shares. Resulting regression models demonstrate the full applicability of party mandate theory to the United States, and they operationalize its U.S. variants concretely.*

Is the laborious process of platform writing and issue refinement, undertaken by U.S. parties every four years, a wasted endeavor? Is voting for one party rather than the other anything more than a symbolic gesture? Is U.S. democracy, in fact, a sham, where electoral endorsements and party government mask the unchanging policy priorities of a bureaucratic, socioeconomic, or power elite? Evidence presented here says *no* to all these questions. Do the policy positions and platform stands taken by U.S. parties and voted on by the electors have any significant impact on government policy? Our evidence returns a resounding *yes.*

What evidence is this, and why should we regard it as significant in the face of some of the most important and intractable questions of democratic theory? The reason is that it relates centrally to the link between party election stands and government policy, which is at the heart of theories of representative democracy in mass society. The specific form taken by such theories is the doctrine of the party mandate, according to which the competing parties offer to the voters different governmental programs between which they can choose. The party which attracts the most votes on this basis then forms the next government, but it is bound (both morally and by fears of retribution at the next election) to carry through the program on which it has been elected.

Nothing is easier than skepticism about mandate theory, whether on grounds of electoral ignorance and indifference (Berelson, Lazarsfeld, and McPhee 1954,

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chap. 2), party selfishness and unresponsiveness (Michels 1962), hidden manipulations by the military-industrial complex (Mills 1956; Lindblom 1977), or the dominance of bureaucracy and the state apparatus (Evans, Rueschemeyer, and Skocpol 1985; Offe 1984). The fact remains that few other justifications of representative democracy are available. If party voting is purely symbolic—if parties really do not transform electoral preferences into policy—why should we have a representative democracy at all, rather than a more efficient non-party-based system?

Our evidence about the relationships involved is based on associations between platform trends and federal spending priorities in the United States over the postwar period. The question of the link between political factors and expenditures has, of course, been extensively discussed in the work on policy outputs, which started off (contrary to our position) by stressing the overwhelming influence of socioeconomic factors on government behavior (Cutright 1965; Dawson and Robinson 1963; Dye 1966; Hofferbert 1966) but subsequently identified a role for political factors—above all, party ideology—within wide socioeconomic constraints (Castles 1982).

Flowing into and invigorating this debate in its later stages has been the second tradition to which our investigation is relevant—speculation and theory about the role played by political parties within representative democracy. With regard to policy outputs, this line of inquiry has reasserted general party influence—and thus the relevance of democratic processes—over what governments do (Castles 1982). Operationally, parties have been credited with certain enduring ideological positions. In the comparative research, policy—compared under one party or party "family," as opposed to another—is posited to reflect the time-invariant influence of those positions. Such studies have commonly employed general indicators of policy or impact such as levels of expenditure, inflation, or unemployment (Cameron 1984; Hibbs 1977; Tufte 1978), sometimes refined by considering the intermediary role of specific government policy instruments such as interest rates (Keman 1984). At the party level these studies have adopted a broad and invariant dichotomy between parties of the "Left" and the "Right," with allowances sometimes made for Christian Democrats in the "Center."

Distinguishable within the general studies of party influence is a particular concern with how far the electoral program of a winning party gets translated into government policy (Ginsberg 1976; Kalegeropoulou 1989; Petry 1988; Pomper 1968; Rallings 1987). Here the basic unit of observation has been specific pledges or promises of some concrete action, as identified in the text of the winning party's platform. Government legislation and administrative actions have then been examined to see to what extent the promises have been carried through. Studies of different countries and time periods have concluded that an impressive proportion of pledges are fully or partially implemented, typically about 70% (Rallings 1987; Rose 1980) but that pledges are usually made in peripheral policy areas (Rallings 1987). For that reason, the pledges may not provide a complete basis for judging party or government compliance with election promises (but see Kalegeropoulou 1989 for an important exception to this).

Clearly, the extent to which election pledges are fulfilled has important bearings on the mandate theory of party democracy, which, as we have noted, forms the basis of our usual justifications of that system, and an important element also in our explanation of how it works (as in Downs 1957; for a comprehensive review and discussion of mandate theory, see Kavanagh 1981). As this theory also forms the conceptual background to our own research, we state its core argument more fully here.

Like most other modern theories of democracy, mandate theory starts from the premise that parties are uniquely important because they alone tie representatives to a particular set of past and promised policies on which voters can make an informed choice in the elections. The voters' ability to do so is important to the theory as a justification of democracy, since the latter is regarded as the only political system giving the ruled a legitimate opportunity to decide what broad lines of policy will be applied to them by the rulers. Other justifications of democracy and elections are of course conceivable. One could agree with Schumpeter (1950) that democracy consists in electors' ability to choose between different sets of leaders rather than the particular policies with which they are associated or that it is the continuing sensitivity of party governments to electors (through continual exposure to polls, for example) which characterizes democracy, rather than a particular choice of policy at elections.

It is, however, difficult to separate either leadership qualities or party sensitivity from policy considerations at election time. A major element in choosing between leaders is what they have done and what they are going to do. And party sensitivity is powerfully reinforced by the periodic practical need for leaders to convince electors of the correctness of what they are doing and propose to do.

One could, of course, argue that voting is essentially retrospective—reacting to past actions of the parties in government rather than to promises about what they *will* do there (Fiorina 1981). Here, again, it is difficult to separate promises from elapsed action, as past record may be the best available pointer to future action and a touchstone by which to judge the credibility of promises.

The latter, in any case, enter into descriptive theories of party competition as the crucial element which parties can manipulate in order to gain votes (Downs 1957). While leadership qualities might also represent an element manipulable by parties, they are not easily represented in the theory. And a total reliance on judgments of past record would subvert the parties' ability to maneuver in policy terms. In effect, Downs's theory and its derivatives *are* party mandate theories, as the whole emphasis is laid on the policy positions put forward by the parties at elections—discounted, certainly, by judgments of how far they have actually carried through promised policies in the past (Downs 1957).

While many glosses and qualifications have been made to the core of the mandate argument, it is difficult to dispense with it entirely in either justifying or describing democracy. Mandate theory, however, should be distinguished from some of the ideas about general party influence on government that have been investigated in the latter policy output literature. The theory does, of course, assert party influence, but it also ties it to a particular instrument, namely, the formal programs endorsed by voters through the electoral success of a party. The theory can thus be refuted by showing that the program is not related to subsequent government policy. In contrast, general party influence could still be exerted if the policy conformed to a subsequent rethinking of the program or to long-standing ideological predispositions not mentioned at the election. We shall return to this point as it affects our operationalization of theory. Our purpose here is to emphasize the central role in mandate theory of policy promises made in public. This piece of the model is not accommodated by most policy output analyses, rendering them inappropriate as checks on the more specific arguments of mandate theory.

Studies of specific pledge fulfillment are clearly checks on mandate theory. (For example, has an election promise to build a particular bridge or to raise pensions by a given amount actually been carried out?) However, they are only partial checks, since they do not cover most of what parties say in their platforms; and a major conclusion is that parties do not often commit themselves to specific pledges in major areas of policy such as inflation, employment, or international relations (Rallings 1987). One can think of candidate Nixon's statement on Vietnam in 1968: "I have a plan"—never specified.

What do parties say about major areas? They refer to the seriousness of a problem, its antecedents, the past record of the party in the area. And they underline the party's concern and determination to take appropriate (unspecified) action.

There is a purpose in all this. Although party strategists are wary of committing themselves to clear courses of action, on which they could be criticized in detail later, they *are* seeking to convince electors, through the media coverage and the comment which the platform will evoke, of the importance of the problem area and its centrality to what the government ought to do in its next term of office. They do this because the issue, if it becomes prominent, should benefit the party by attracting a net inflow of voters. There is considerable evidence for such a "salience theory of party competition," in which parties "own" certain issues (in the sense of attracting a net inflow of votes when they become prominent) and compete by trying to emphasize whichever of "their" issues are appropriate in the election (Budge and Farlie 1983; Budge, Robertson, and Hearl 1987; Hofferbert and Klingemann 1988; Robertson 1976; Sarlvik and Crewe 1983).

In this sense, topics emphasized by party platforms constitute implicit commitments to greater effort in the area if elected to government, at the expense of issues downgraded in the platform. At a minimum this involves changing budgeted expenditures at the margin, as few courses of action can be pursued without some expense. An investigation of whether the changing emphases of party platforms relate to changing government expenditure later on is thus an appropriate test of mandate theory. This strategy fleshes out earlier studies of pledge fulfillment and has the advantage of covering central themes of the program rather than marginal parts. Looking at emphases in a particular election (which may differ substantially from those made before and after) also provides us with a more sensitive indicator of party concerns than an invariant ideological characterization of parties as "reformist" or "conservative"—though, of course, such enduring ideological biases may exert an independent effect, which we investigate below (model 6).

Data

At the start of this investigation we already had access to a comprehensive coding of U.S. platform emphases from 1948 through 1984, undertaken as part of a comparative spatial investigation of party system "dimensions" and movement on them over the postwar period (Budge, Robertson, and Hearl 1987). Each sentence of the platforms had been coded into one of 54 detailed categories, grouped under seven broad themes (foreign, democracy and civil rights, government, economy, welfare and quality of life, "fabric of society," and social groups). Examples of detailed categories are social services, social justice, education, environmental protection, law and order, and so on (see Tables 3–6). The categories and coding procedures are described in detail in Budge, Robertson, and Hearl 1987 (app. B). The salience theory of party competition that underlies this coding is to a considerable extent validated by the plausible results obtained in the original analyses.

On the expenditure side the data consist of broad categories of federal spending reported by the Office of Management and Budget. We use these data in the form of percentage of the total budget for individual categories. This is the most direct way of measuring policy *priorities,* whereas other measures, such as raw expenditures or percentage of GNP, either give misleading impressions or bring in factors extraneous to policy *priorities.* For example, while defense has increased its total expenditure over the period, it has gone down as a percentage of the budget, reflecting its downgrading as a priority relative to social services (up to 1980). Measuring expenditure as a percentage of GNP allows it to be affected by the expansion or contraction of total government revenue. Revenue is the subject of decisions rather distant from those involved in allocating expenditure between areas, which is what we want to catch in the notion of policy priorities. Additionally, percentage of GNP would be affected by extraneous factors such as the buoyancy or otherwise of the economy. In the subsequent analyses, therefore, we rely exclusively on each policy area's percentage of end-of-fiscal-year expenditures as reflecting policy priorities most directly.

As the coding of both platform emphases and expenditures had in a sense been present and designed for other, though related, purposes than those we pursue here, considerable experimentation in matching and grouping was necessary before we were sure we had an optimal correspondence between categories on both sides. After trying alternatives, we decided against aggregating from among the original 54 content analytic categories to "fit" them better to expenditure areas. To explain policy priorities, therefore, we relate the percentage of expenditures for each of 14 policy areas to the percentage for specific categories in each quadrennium's party platforms. The emphases selected to match up to expenditures were chosen, first on the basis of face validity as substantively relevant and, second, on the basis of experimental computations. The matches we were able to make gave us a fair cross section of programmatic categories and a near full set of expenditure areas. The impressive correlations between them indicate that the matching is plausible. The matched platform categories and expenditure areas are listed in Table 1.

Clearly these matchings and the categories involved are somewhat broad and gross, and they could have been substantially refined had data collection and coding been designed specifically for this purpose. From the start of the project, the choice of the budget categories has not been in our hands. True, platform categories could have been changed by a massive recoding; but this would have ruled out comparisons with other countries, which we hope to make later; and it would only really have been justified if our initial investigation had not yielded the good results it did. In a sense the gross matches we do make are a conservative element in the research design since they operate to reduce rather than to inflate coefficients. When these emerge even from the approximations we are forced to make they must indeed be strong.

Using such longitudinal data in aggregated form means that we use trends over the whole postwar period to establish relationships, rather than breaking them down to see if, for example, developments in the later postwar period differed from those which took place earlier. Limitations on the numbers of cases render it unwise to apply statistical models of the same complexity as those described below to 18 cases or less. We have, however, checked plots of relationships over time and examined series closely for signs of natural breaks that might divide the postwar period into two parts. None appear. Indeed the postwar period as a whole appears as more of a unity than any of the

Table 1. Matching of Party Platform Content Categories and Federal Spending Categories

Spending Area[a]	Party Program Area[b]
Human Services	
Social security	social justice
Health and medicare	social justice
Income security (welfare)	social justice
Education & employment services	education
Defense and foreign policy	
Defense	foreign special relations
International affairs	foreign special relations
Physical resources	
Agriculture	agriculture
Energy	technology & infrastructure
Commerce & housing credits	technology & infrastructure
Community & regional development	technology & infrastructure
Transportation	technology & infrastructure
Natural resources & environment	environmental protection
Governance	
General government	government efficiency
Administration of justice	law & order

[a] U.S. Office of Management and Budget 1988.

[b] For more complete description of coding procedures and the content analytic categories for platform emphases, see Budge, Robertson, and Hearl 1987.

(inherently artificial) divisions we could break it down to. The only reason for breaking down the period would be to improve or refine the relationships we find. As these are quite strong anyway and unaffected on the whole by short-term deviations, there seems no point at this stage in making the analysis more complex.

Operationalizing Mandate Theories in the U.S. Context

Mandate theory sees parties as presenting to the electorate programs that characterize the current political situation in often radically different ways. The election yields a victory by the party whose package best reflects electoral preference (or which more generally manages to convince the majority that its characterization of the political situation is best). In turn, the mechanisms of government allow the winning party to translate its priorities into policy. These expectations are (drastically) summarized in Table 2.

Table 2, of course, stands as an ideal model of the processes involved. Policy translation, particularly at the governmental level, will be limited by institutional arrangements and coalition governments in most of the European democracies and by separation of powers in the United States. Indeed, party unity between a coequal U.S. national legislature and the president is more an exception than the rule. The Democrats have controlled the House of Representatives for all but two years in the period 1948–88 (1953–55 being the exception). For all but 1953–55 and 1981–87 they have also controlled the Senate. (This imbalance in control also affects attempts to incorporate party alternation in Congress into our models.) In effect Congress has been a

Table 2. Outline of Pure Mandate Theory of Party Emphases, Electoral Choice, and Policy Enactment

Preelection Period	→	Election	→	Postelection Period
Party A:	:	Electorate Choose	:	Government Formed by
Issue x = +	:	Party A	:	Party A Enacts Policies:
Issue y = −	:		:	
Issue z = +	:		:	Policy x = +
	:		:	Policy y = −
Party B:	:		:	Policy z = +
Issue x = −	:		:	
Issue y = +	:		:	
Issue z = −	:		:	

Democratic bastion, while the presidency has been Republican for over half of the same period. In the 1988 elections the split was intensified with (small) Democratic gains in House and Senate while the presidency continued to be held by a Republican.

Separation of powers indeed could reinforce common impressions of the U.S. parties as pragmatic, disunited, nonprogrammatic gangs of officeseekers. It could argue against any applicability of the mandate theory at all. Such, certainly, has been the dominant message of research examining linkages between constituency attitudes and attitudes and behavior of members of Congress (Miller and Stokes 1963; Sullivan and O'Conner 1972). There is, however, an impressive body of research indicating that the major parties' platforms have approximated the preelection prerequisites of the mandate model (Budge, Robertson, and Hearl 1987, chap. 3; Ginsberg 1976; Krukones 1984; Pomper 1968). Pomper's work additionally shows that the legislative agendas of the two parties reflect, in the main, the issue positions taken in their platforms—though he is unable, within the bounds of his evidence, to examine the linkage to regulations or budget allocations.

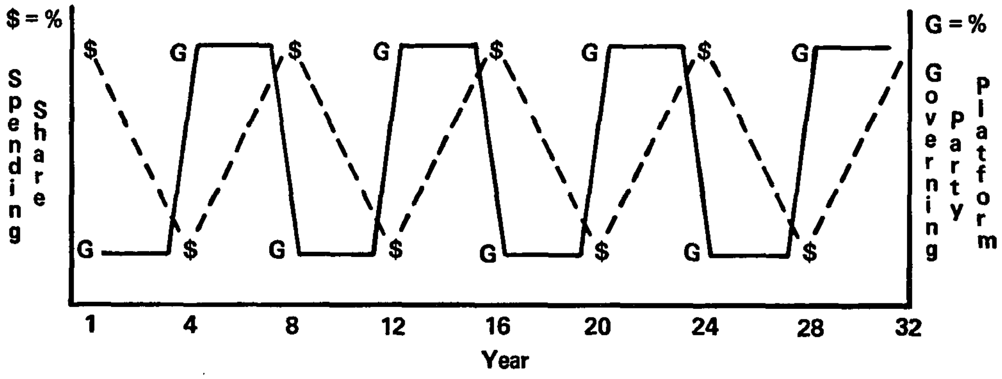
Certainly if such links *are* found for U.S. parties, traditionally perceived as loose and nonprogrammatic, that discovery would constitute forceful evidence for the applicability of mandate theory elsewhere. Clearly, however, we have to take cognizance of (1) the sharing of power in the U.S. context and (2) the permanent "preserve" of the Democrats at top levels of government, due to their domination of Congress.

Both major parties often have a "mandate" of sorts in the United States. And the statistical models presented below take cognizance of this point, even though they follow broadly the process postulated in Table 2.

Before discussing detailed models it will help to present the basic relationships graphically. Figure 1 shows examples of a positive correlation of ruling party quadrennial platform emphases for a particular policy area lagged on percentage expenditure in that area. This relationship would hold whichever way the lines rotated in space, though for simplicity no long-term upward or downward trend in either expenditures or emphases is shown in the figure. The regression and correlation coefficients presented in subsequent tables relate to relationships of this basic type over time.

Customary views of party platform irrelevance hardly allow for such linkage. The process of platform preparation, however, may yield a crystallization of the agenda of key elites and collective ac-

Figure 1. Hypothesized Relationship between Governing Party Quadrennial Platform Policy Emphasis and Annual Spending Share



$ = Expenditure percentage for policy "X"
G = Governing party emphasis on policy "X"

tors. That process may yield decisions the costs of which need not be borne twice—once before, and once after, the election. Mandate theory requires, in the postelection period first, veracity and second, institutional capacity.

It can be argued, once superficial disbelief is suspended, that changing party priorities are the most obvious factors in the policy environment to which we can look for an explanation of changing policy. So the onus is really on opponents of party mandate theory to show they are *not* influential rather than on us to show that they are, that is, if a prima facie case exists, as it does with our data.

To account for the timing of the budget and spending cycle, our expenditure data are lagged by two years; that is, 1948 platform emphases are linked to 1950 expenditures, 1952 emphases to 1954 spending, and so on. As in most jurisdictions, the federal budget cycle (which would be the first step in platform implementation) is a full year, followed by another year to spend appropriated funds. No U.S. president coming to power can hope to alter an annual spending record substantially in less than two years. We use expenditures rather than *budgets* due to the variety of entry points between formal budgeting and actual outlay.

Although not introduced expressly to demonstrate a causal effect, the consequence of lagging is to suggest that there is one, with temporally antecedent emphases being linked to later expenditure. Various tests conducted on the effects of time as an influence both on emphases and expenditures demonstrate a limited effect that by no means detracts from the independent influence of platform emphases on spending shares.

In any case, it is enough for mandate theory that expenditures should *correspond* to emphases rather than that there should *necessarily* be a cause-and-effect relationship between them. If, for example, bureaucrats always anticipated winning party emphases and started tailoring expenditures to them without being required to do so or responded spontaneously to problems exercising politicians, this does not undermine party democracy—quite the reverse. So there is no theoretical reason for a correlation of emphases and expenditures to be undervalued relative to a full cause-and-effect impact.

The second methodological question we should consider here is how we relate platform emphases, written only quadrennially and approved then by the party conventions, to *annual* budgetary data.

The answer is that we record the same emphases for each of the three following years, until they are replaced by the subsequent platform. Thus, the 1948 emphases are recorded also for 1949, 1950, and 1951; and so with the other years. This is not simply a "double-barreled data extender" designed to increase the number of cases, though it has this effect. We record data in this way because the theory regards programmatic emphases as rightfully determining for each year in which the party holds power and makes up a separate budget, as suggested in Figure 1. To the extent that priorities change and programmatic emphases are not reflected in spending (subject to the lag discussed above) the theory is contradicted.

Arranging our data in this way, with the two-year lag, has the paradoxical effect that Truman's platform priorities of 1948 are still being related to expenditures in 1954, up to 23 months after Eisenhower's inauguration. Given the delays and lags discussed earlier this is plausible. We have experimented with cross-sectional analyses of the data, as well as relating emphases only to the middle and end years of a presidency, when "purer" programmitic influences should be at work. None of these alternative analyses affects relationships much. The remarkable thing about the results presented below is that they *do* produce strong relationships, confirming our intuitions about the process at work.

Our number of cases, therefore, is 37 for all variables, with each year from 1948–85 inclusive appearing as a separate case. The last case is constituted by the final 1987 fiscal year outlays linked to the 1985 platform emphases.

Regression Models

Fitting mandate theory to a situation where parties may share responsibility for policy means that a wider range of possible relationships must be envisaged than those of the "idealized" version in Figure 1 or Table 2. Variations on six models capture what we see as the major operationalizations of the theory, in terms of spending priorities and party program emphases, in a situation like that in the United States:

1. Competitive model: $Y = \alpha + \beta R + \beta D$
2. Complementary model: $Y = \alpha + \beta(R + D)$
3. Consensus model: $Y = \alpha + \beta(R \times D)$
4. Control model: $Y = \alpha + \beta(R \times P_R) + \beta(D \times P_D)$
5. General program model: $Y = \alpha - \beta R - \beta D + \beta(R \times P_R) + \beta(D \times P_D)$
6. General partisan influence model: $Y = \alpha - \beta R - \beta D + \beta(R \times P_R) + \beta(D \times P_D) + R$

where Y = % federal spending by functional area (lagged 2 years); R = % Republican platform devoted to theme matched to spending area Y; D = % Democratic platform devoted to theme matched to spending area Y; P_R, P_D = dummy variable (1 = control of presidency, 0 = no control of presidency); R = dummy variable, with a value of one for a Republican president and a zero for a Democrat.

Model 1, the *competitive model,* is based on the hypothesis that parties compete within the mutually defined issue spaces. The outcome of competition is measured in a reinforcing or contradicting effect, irrespective of who holds what offices. This model highlights the kind of power sharing associated with the separation of powers in the United States.

A variation on the competition model can be identified as the *hegemony model,* where one party's emphases consistently dominate, irrespective of office holding. Thus, it might be hypothesized that the Democrats in the United States would see their strongest emphases consistently reflected in policy, regardless of Republican priorities, because of the former's congressionally based bargaining power. Where Republicans felt strongly, they too might exert such an effect through control of the presidency and congressional bargaining power. The presence of hegemony is revealed where one of the terms of the equation of model 1 either drops out or becomes negative (indicating that platform emphases tend to go in the opposite direction to expenditures).

Model 2, the *complementary model,* hypothesizes the kind of mutual accommodation of interests envisioned in the *Federalist Papers.* Policy will reflect party programs only to the extent that both parties show complementary trends in their programmatic emphases.

Model 3, the *consensus model,* hypothesizes that agreement in relative programmatic emphases between the parties has a disproportionate impact on policy. The result of consensus is multiplicative. Or, put the other way, the absence of consensus, given the U.S. system of checked power, is exponentially consequential in blocking a party's capacity to translate platform emphases into budgetary priorities.

None of these three models requires unified partisan control of the Congress and presidency. Given the historic patterns of shared power, that omission ought not to disqualify any of the models on its face. In the U.S. *both* parties may have an electoral mandate. On the other hand, it is true that twentieth-century presidents exercise major control over the policy agenda, not only through formal instruments but also through the indirect instruments of public visibility and leadership. The impact of presidential party is accommodated by model 4, the *control model.* To compute this model, the data are arranged so that the interactive variables (party platform × control of presidency) receive a value of 0 when the party is not in the White House and a value equal to the percentage emphasis when in the presidency. The hypothesis here is that each party's program will determine policy, but only when that party occupies the executive position.

Model 5 (the *general program model)* modifies this, contrasting an assumed positive relationship between program emphases and expenditures when a party holds the presidency with an expected negative relationship when it is out of office (see Figure 1). The contrast in parameter estimates for the untransformed term (R or D) and the matching interactive term ($R \times P$) or ($D \times P$) shows the relative gain in programmatic impact from holding the presidency. Likewise the contrast across parties between R versus ($D \times Dp$) or D versus ($R \times Rp$) indicates the simultaneous effect of each presidential administration and its opposition. When the Republicans have the White House, the trend in policy priorities (e.g., percentage of expenditures) matches Republican platform emphases. The Democratic platform is irrelevant (or perhaps negative). Under a Democratic president, the trend in policy priorities matches Democratic platform emphases, with the Republican platform is irrelevant or negative. This model is the one that corresponds most closely to the idealized mandate theory of Figure 1 and Table 2.

We pointed out above that comparative policy research has customarily examined the raw impact of party control, regardless of programmatic content. When the longitudinal relationship between variance in party control (e.g., percentage "left" party seats or a dummy variable for "left" control of government) and variance in policy outputs is investigated, it is either explicitly or implicitly assumed that any association is due to the policy impact of enduring partisan principles. Thus, if we investigate the relationship between Democratic control of the presidency and priority for human services, any positive results demonstrate some sort of enduring commitment of the Democrats, in contrast to the Republicans, to the area of human services. Our current research suggests that the actual commitment is variable, even though the mean may be higher for the Democrats than for the Republicans.

One additional term will enable us to sort out this enduring partisan or ideological impact from the impact of party platforms, in and out of the White House. This is the final variant on the basic party program → policy priority theme. The general program model (model 5) includes a negative term for each party when out of the presidency and a multiplicative term when in. If we add a dummy variable for control itself, we can measure independently out-party program effect, in-party translation of program into output, and the impact of enduring partisan differences, as indicated by the equation of model 6, the *general partisan influence model.*

The last term, R, is a dummy variable, with a value of one for a Republican president and zero for a Democrat. It can be seen as an addition to or subtraction from the constant (α) of the equation of model 5. The dummy variable can take on only one value other than zero, namely, one. Therefore, a unit increase in that variable is always the same, that is, one, or, in effect, a shift in the constant, so far as the other terms of the equation are concerned.

Analysis

In the U.S. system of separated executive and legislative powers, with divided party control in more than half of the years considered, it is clear that in many of the equations we are not examining a sequence of "in" and "out" parties but rather mixes of power, with neither party ever relinquishing substantial electorally based influence. This is especially evident in the assumptions of the first three models (competitive, complementary, and consensus). In point of fact, neither of the latter two models (2 and 3) fit the data as well as the competitive model, so specific statistics are not reported for these. This result is balanced by an equally poor performance of the contrasting control model, which discounts program emphases unless the party has control of the presidency (model 4). In all expenditure areas it was outperformed by both the general program and general partisan influence models, which both incorporate relationships for parties in and out of the White House.

Some question arises as to whether we should not have "detrended" the data, that is, removed any effect of common time trends. However, a crude imposition of such a smoothing strategy would bury much that we are interested in revealing. An analogy to physical training is appropriate. The more one trains, the faster one can run. There is a high longitudinal correlation between training and speed. Control for time, however, and the relationship disappears, suggesting no "true" relationship between training and speed.

One detrends only when (1) one has no idea what is driving the longitudinal relationship or (2) there is a specific known but unmeasured time-associated variable affecting both terms in the equation. In the case at hand, neither of these limiting conditions is present.

The analysis may be conveniently and usefully broken into four broad, substantive sets. We divide our discussion into major areas of expenditure as follows: human services, defense and foreign policy, physical resources, and a set of special categories (that don't fit neatly into the other substantive areas).

Human Services

Table 3 presents unstandardized regression coefficients for the four separate spending areas within this broad rubric. We also present results for aggregated emphases and expenditures over the whole area, which appear to go together rather well.[1] We start analysis of each policy with the basic competition model; go on to the general program model, where it improves the results (as it does in all cases here); then on to the general partisan influence model, where it offers further improvement (here only in the case of education). (In the tables, the predicted relationship is indicated by the signs before each β in the column headings, with the actual relationship indicated by minus signs when negative or no sign when positive.

The reason that models 2–4 fail to improve the fit is the remarkable success of the general program model (model 5). R-squareds of .88 or even of .49 and .62 are uncommon in political science, still more so the stratospheric .94 attained by the general partisan influence model in the case of education. Here even the simple competition model attains an impressive .75, due mainly to the high positive relationship between Democratic em-

Table 3. Models of Platform Impact on Expenditure Priorities for Human Services, 1948–85

Expenditure Priority	Platform Predictor	R^2	α	βR	βD	$\beta(R \times P_R)$	$\beta(D \times P_D)$	R
		Model 1: Competitive						
Social security	Social justice	.41	17.20	-4.4	1.4*			
Health & medicare	Social justice	.09	6.3	-1.3*	-.01			
Income security	social justice	.07	11.03	-1.3*	-.37			
Education & employment services	Education	.75	-.58	.04	1.0*			
Aggregate models: human services	Social justice & education	.24	16.34	-.65	3.36*			
		Model 5: General Program						
Social security	Social justice	.62	22.48	-7.3*	-3.6*	9.7*	2.3*	
Health & medicare	Social justice	.49	11.16	-4.1*	-3.9*	7.6	.60	
Income security	Social justice	.17	13.22	-2.2*	-.81	2.3	-.96	
Education & employment services	Education	.88	-.29	-.12*	.66*	.50*	.55*	
Aggregate models: human services	Social justice & education	.33	13.45	-1.34	2.1	3.4	3.5*	
		Model 6: General Partisan Influence (With Republican President Dummy Variable)						
Social security	Social justice							
Health & medicare	Social justice							
Income security	Social justice							
Education & employment services	Education	.94	.47	.18*	1.1*	.47*	-.08	-2.6*
Aggregate models: human services	Social justice & education							

Note: Hypothesized direction is indicated on p. 117 for each model. All R-squareds are adjusted for the number of variables in the equation.

$^*p \geq .05$.

phases and expenditures. Given the simple nature of the program coding and the somewhat intuitive matching of emphases to expenditure areas, we would have gladly settled for less than this.

Turning to the performance of each respective model, there are (perhaps deceptive) patterns in even the simple competition model. Republican emphases appear negatively reflected in most human service spending. The correlation of Republican total social justice and education emphases with time is −.04. By *increasing* the human services share of outlays, Republican administrations (especially in the Nixon-Ford years), acted contrary to the expectations one might have of the more conservative party and in contrast to their modest *declining* attention to social justice considerations in their platforms (significant βs = −4.4, −1.3, −1.3 for social security, health-medicare, and income security, respectively, as predicted by program emphases on social justice); that is, Republican administrations appear generally to have increased human services' share, in contrast to the trend in platform emphases. The statistical significance disappears, however, when all human service areas are aggregated. Interestingly, the Democratic platforms predicted significantly only social security and education-employment services (principally special education projects, vocational education, and research—small expenditures that perhaps for that reason were open to congressional influence). But Democratic emphasis did strongly predict the total human services trend (3.36). There is a hint, especially in the totals, of a hegemonic relationship here, with minimum Republican influence. This suggestion is muted, however, when the more complex models of human services priorities are examined.

Much that seems anomalous in the realm of human services becomes clearer when the general program model (model 5) is examined. By way of reminder, the terms of the model can be read as: constant − Republicans out of the presidency − Democrats out of the presidency + Republicans in the presidency + Democrats in the presidency. The contrast in hypothesized signs of the βs goes back to Table 2. In terms of this model we expect a party's emphasis to anticipate expenditure trends when it holds the presidency but possibly to run counter to them when it does not.

The White House multiplier increases the explained variance in all four areas of human services policy, with only one below 49% (income security, which is mainly welfare and related income-transfer programs [R^2 = .17]). The social justice emphasis of platforms of the *non*-presidential parties are consistently negatively related to human services priorities, with the platform of the president's party positively and significantly related to policy priorities in most instances. Thus, when they are *out* of the White House, for example, for every percentage change in the proportion of the Republican platforms devoted to social justice themes, we could have expected a 7.3% shift in social security's share of federal spending in the opposite direction. But when the Republicans are *in* the White House, each 1% up or down on social justice is directly reflected in a 9.7% shift in social security's share of outlays.

The pattern holds, rather more modestly, for total aggregated human services, with the general program model explaining 33% of the variance. However, some precision is clearly lost in the aggregation process, since only the Democratic-White House term retains statistical significance.

Although the federal government is the minor actor in education, compared to states and localities, it is one of the most interesting areas in terms of its relevance to party platforms. Our speculation would be that the purposes to which federal education funds are devoted are far less institutionalized and rigid than is the case with social security, health, or welfare. Education is also the only human service area in which a significant improvement in explanatory power is gained by adding the raw partisanship variable (that is, the dummy variable for Republican presidential occupancy, model 6). When both party platforms' emphases on education, in and out of the White House, plus the partisanship dummy variable are included, the explained variance goes to 94%. Republican control has contributed a constant of 2.6% decrease in education's share of federal spending, irrespective of variation in the Republican platforms. A percentage of Republican platforms' emphasis on education contributes .47 up or down when Republicans hold the presidency; and while out of the White House, the Democrats have used their congressional base to insert another 1.1% for every percentage of their platform emphasis on education.

Thus we may conclude that

1. Occupancy of the White House, as accommodated by the general program model, connects platform emphases to the priorities in federal spending for most areas of human services.
2. In one specific area (education) congressional influence is reflected in continuing effect of the Democratic platform, even out of the White House.
3. Human services policies are sufficiently differentiated in their reflection of party platforms to warrant disaggregated analysis, with education the least comparable to the general pattern.
4. Enduring partisan differences are generally less consequential for policy priorities than are trends in formal party platforms (again with some deviation in education).

Defense and Foreign Policy

Defense and international spending priorities, as predicted by emphases on special foreign relations (most often NATO), offer grounds for interesting speculation about the platform→policy priority linkage. As in human services, the general program model provides a notably better fit than the simple competition model, increasing the explained variance from .27 and .31 to .36 and .47, for defense and international affairs spending shares respectively. And, as with human services, a mix of potentially misleading relationships in the competition model become clear under the more complex model, with the general program model conforming in ideal fashion to its hypothesized form. For every percentage shift of the Republican platform devoted to special foreign relationships, with a Republican in the White House, defense gets a corresponding change of 6.6% in its share of federal expenditure. Likewise, for every percentage shift in the Democratic platform devoted to defense, with a Democrat in the White House, there is a change of 7.1% in the defense share.

Expenditures on international affairs are responsive to exactly the same model, with only the absolute magnitudes of paramater estimates differing.

Similarly, when out of the presidency, each party sees its platform emphases negatively reflected in defense and international shares. The enduring partisanship term (*R*) adds nothing to either area or to the aggregation of both. Aggregating international and defense does no damage at all to the general program model.

We conclude that

1. Occupancy of the White House, as accommodated by the general program model, links platform emphases and priorities in federal spending on defense and international affairs.
2. Enduring party differences are inconsequential for defense and international shares, relative to the impact of party platform content.
3. Defense and the broad set of inter-

Table 4. Models of Platform Impact on Expenditure Priorities for Defense and Foreign Relations, 1948–85

		Model 1: Competitive					
Expenditure Priority	Platform Predictor	R^2	α	βR	βD	$\beta(R \times P_R)$	$\beta(D \times P_D)$
Defense	Special foreign relations	.27	43.8	-3.5*	3.9*		
International affairs	Special foreign relations	.31	2.1	-.34*	-.74*		
Aggregate models: defense & international affairs	Special foreign relations	.32	45.9	-3.81*	4.63		
		Model 5: General Program					
Defense	Special foreign relations	.36	53.9	-9.0	-.20*	6.6*	7.1*
International affairs	Special foreign relations	.47	4.1	-.94*	-.32	.84*	1.3*
Aggregate models: defense & international affairs	Special foreign relations	.42	58.0	-9.95*	-.52	7.4*	8.4*

*$p \geq .05$.

national affairs expenditures are subject to common party and presidential forces, suggesting that for many analytical purposes they may be aggregated without loss of precision.

Physical Resources

We have included spending for natural resources and environment, on energy, transportation, community and regional development, and on commerce and housing under the rubric *physical resources.* In all but natural resources and environment, we link platform emphases on "technology and infrastructure" to physical resources priorities. As with the previous two general categories, it helps to know which party was in the White House in searching for the linkages between party platforms and policy priorities for physical resources. Seven of the 10 possible interactive terms ($R \times P_R$ and $D \times P_D$) across the five policy fields are statistically significant, indicating consequential platform contributions. That it is only seven, however, is reason for caution about concentrating too heavily on platform impact.

In three of the five categories, there is a significant impact of enduring partisan differences (i.e., R, the dummy variable for Republican presidency). In those cases, that term was significant—and bigger than any of the competing terms. In natural resources–environment, energy, and transportation spending the most powerful element is simply which party controls the White House. With a Republican president, irrespective of party platforms, transportation gets a 2.3% larger slice of the budget than under the Democrats. The Democrats, likewise, boost the share for energy by .85 and for natural resources-environment by .77. All these effects are statistically significant.

As noted above, the interactive $R \times P_R$ and $D \times P_D$ terms, measuring platform impact of each party when controlling the presidency, are significant for both parties in model 5 (or model 6, when the R term adds significantly to the R-squared) for natural resources–environment, energy, transportation, and commerce and housing. In the first case, where we have re-

Table 5. Models of Platform Impact on Expenditure Priorities for Physical Resources, 1948–85

Expenditure Priority	Platform Predictor	R^2	α	βR	βD	$\beta(R\times P_R)$	$\beta(D\times P_D)$	R
		Model 1: Competitive						
Natural resources & environment	Environmental protection	.17	2.1	.07*	–.09*			
Energy	Technology & infrastructure	.24	.74	.11*	–.06*			
Transportation	Technology & infrastructure	.10	3.0	.47*	–.05*			
Community & regional development	Technology & infrastructure	.51	.82	.20*	–.12*			
Commerce & housing credits	Technology & infrastructure	.13	2.0	.01	–.10			
Aggregate models: physical resources	Environmental protection & technology & infrastructure	.40	8.6	.34*	.20*			
		Model 5: General Program						
Natural resources & environment	Environmental protection	.29	1.9	–.07	.11*	.17*	.18*	
Energy	Technology & infrastructure	.42	.66	.20*	–.04	–.14*	–.04	
Transportation	Technology & infrastructure	.47	2.4	.08	–.13*	.21*	.29*	
Community & regional development	Technology & infrastructure	.61	.59	.23*	–.13*	–.01	.07	
Commerce & housing credits	Technology & intrastructure	.21	1.8	–.13*	–.16*	–.23*	.17*	
Aggregate models: physical resources	Environmental protection & technology & infrastructure	.57	7.2	.38	–.28*	–.02	.26*	
		Model 6: General Partisan Influence (With Republican President Dummy Variable)						
Natural resources & environment	Environmental protection	.46	2.2	–.06	–.07*	.23*	.08*	–.77*
Energy	Technology & infrastructure	.51	1.2	.20*	–.02	–.12*	–.14*	–.85*
Transportation	Technology & infrastructure	.70	.75	.08*	–.17*	.16*	.58*	2.3*
Community & regional development	Technology & infrastructure							
Commerce & housing credits	Technology & infrastructure							
Aggregate models: physical resources	Environmental protection & technology & infrastructure							

$^{*}p \geq .05$.

lated expenditures to platform emphases on environmental protection, the general partisan influence model (model 6) fits well, with all signs in the expected directions (although the $D \times P_D$ term is not statistically significant). In three of the other four cases (where expenditures have been related to emphasis on technology and infrastructure) the more complex models do not fit entirely as predicted. The signs of the βs in many cases are not as expected, with parties' emphases when they are "out" relating more to expenditures than their emphases when they are "in." These may be put down to highly skewed expenditure patterns in the case of energy, but the set of policies clearly demands more intensive examination.

Commerce and housing credits includes a wide range of activities and is generally less responsive as a category to party emphases (model 5, $R^2 = .21$), in these instances emphasis on technology and infrastructure, although the four signs are all significant and three are in the predicted direction.

The simple competitive model (model 1) fits very well in the area of community and regional development ($R^2 = .51$), with the more complex model 5 adding only 10% to R-squared. It tends, indeed, toward a Republican hegemony model, since the Republican, rather than the Democratic, emphases covary positively with expenditures.

The variety of findings across physical resources policy areas makes us skeptical of the wisdom of aggregating the fields together, even though the "total" equation does yield R-squareds of .40 and .57 for the competitive and general program models respectively. The fact that the latter deviates from the hypothesized direction is more a matter for pause than wonder, reinforcing skepticism about the wisdom of aggregating policy areas according to the conveniences of federal bookkeepers rather than by some more theoretically refined rationale.

While the findings for physical resources are not as resounding as for either human services or defense-foreign policy, they are nonetheless worthy of attention. Party programs matter. When combined with indicators of control of the presidency, R-squareds up to .70 are obtained, with most in the neighborhood of .50, with the aggregate general program model explaining 57% of physical resources spending priorities. The following conclusions seem warranted:

1. Unlike human services or defense-international spending priorities, enduring partisan differences (regardless of temporal variance in platform content) are often very consequential for physical resources shares.
2. Platform trends are sometimes consequential for certain policy areas, especially when coupled with control of the presidency.
3. There is evidence of definite (Republican) hegemony in selected areas of physical resources policy priorities.
4. At this stage of inquiry, caution should be exercised in overaggregating physical resources policies.

Special Categories

General government, which includes principally federal bureaucratic costs, is grouped with administration of justice and agriculture largely because the form of relationships found in all three cases resemble each other, not because of substantive resemblances. Agriculture properly belongs on its own because of the support nature of expenditures in the area. The simplest competition (or even hegemonic) models fit these areas nearly as well as, or better than, the more complex ones. There is a technical reason for this in the cases of agriculture and the administration of justice—multicollinearity. Republican and Democratic emphases on law and order correlate at .86 over time;

Table 6. Models of Platform Impact on Expenditure Priorities for Special Categories, 1948–85

Expenditure Priority	Platform Predictor	R^2	α	βR	βD	$\beta(R \times P_R)$	$\beta(D \times P_D)$	R
		Model 1: Competitive						
General government	Governmental efficiency	.54	1.5	.08*	−.19*			
Administration of justice	Law & order	.78	.41	.02*	.65*			
Agriculture		.25	1.3	.08	.18*			
		Model 5: General Program						
General government	Governmental efficiency	.55	1.8	.02	.09	−.31*	.11	
Administration of justice	Law & order				[multicollinearity]			
Agriculture					[multicollinearity]			
		Model 6: General Partisan Influence (With Republican President Dummy Variable)						
General government	Governmental efficiency	.60	2.2	−.08	−.05	.15	−.17	-1.3*
Administration of justice	Law & order							
Agriculture								

*$p \geq .05$.

emphases on agriculture, at .64. There is, therefore, little one can do to separate out the differential impact of each, beyond noting the overall R-squareds of .78 and .25. Even the latter is impressive. Given these figures, one certainly cannot conclude that party platforms are irrelevant, merely that they are indistinguishable from one another—though there is a temptation to cut the Gordian Knot by opting for a single party, Democratic hegemony in each area, which would be supported by the regression coefficients (so far as they go).

The simple competition model seems to offer an attractive explanation for general government's share of federal spending, though here the Republican emphases (.08) seems to fit expectations better than the Democratic (−.19). Further exploration, however, argues for stressing the general partisan influence model, even though it is a much less parsimonious explanation, with only a 6% gain in R-squared (.60 versus .54 for the competitive model). With White House control *and* long-term simple partisan impact jointly measured against platform emphases, the latter lose out. Republican control (R) has meant 1.3% less in expenditures on general government. Knowing the party in office is far more important for predicting these costs than is knowing the trends in party platform concerns.

General Results of the Analysis

The specific ways in which program emphases affect spending clearly vary from one policy to another—a finding anticipated and allowed for in the different models put forward. The analysis as a whole, however, supports a series of general conclusions:

1. Most federal spending priorities in the United States are quite closely linked to prior party platform emphases in the postwar period.
2. In a majority of areas, control of the presidency significantly enhances the likelihood that spending priorities will reflect the trends in platform content.
3. Enduring partisan differences are generally less consequential for spending priorities than are current platform emphases; but the effects of such partisan differences are of major importance in particular policy arenas, most notably in physical resources and in general governmental spending priorities.

Conclusions: Mandate Theory and General Party Influence

Without belaboring these results, we briefly assess here the bearing of our analysis on some of the theoretical points raised. We noted that the U.S. case, because of its peculiarities, offers a tough test for mandate theory. It has passed the test. If U.S. voters want to know how their taxes are going to get spent, they would do well to look at the trends in attention to the platform of the president's party. If they want to effect alternative packages of priorities with their votes, they had better select the party whose platform, on balance, is moving in the desired direction.

The most striking finding is that a stripped-down mandate theory, emphasizing the policy impact of the party in government (see Table 2) and the irrelevance or exclusion of the opposition, works so well in the U.S. case, in spite of the separation and diffusion of power. Model 5, the general program model, positing dominance by the platform of the occupant of the White House, fits most areas best.

We have perhaps not been fully able to assess congressional influence over expenditure shares, due mainly to the difficulty of creating dummy variables comparable to the presidential ones in a period when Democrats failed to control both houses of Congress for only 1953–55 and 1981–87. Apart from the brevity of periods of less than full Democratic control, these periods fall awkwardly near the beginning and end of the postwar era; and their effects are statistically indistinguishable from those of time. (We have experimented, and results are inconclusive.)

Clearly, however, the congressional parties have a mandate too, which is caught to some extent in our models where the "out" presidential party platform emphases continue to be related to expenditures. It is also evident in the fit of the competition model and its special version, the hegemonic model, in some areas. Given the U.S. situation, this does not contradict the mandate theory—it simply refines it. It is a result that we might expect as well from the diffusion of mandates through coalition governments in European parliamentary systems.

The comparative policy literature reviewed in the opening section has been concerned with the general influence of parties on government (usually measured by averages of officeholding over several years) rather than with the narrower question of what their mandate was and whether it had an effect. Our model 6, general partisan influence, incorporated the dummy (*R*) for Republican tenure of the presidency. Its importance in certain policy areas, along with the programmatic emphases of the party holding the presidency, certainly demonstrates partisan influence on government—if not dominance of it.

More broadly, while disconfirmation of mandate theories would not disprove general ideas about the primacy of party in government (expenditures could still respond to current party preoccupations rather than ones presented in the earlier electoral program), the success of mandate theories certainly upholds these ideas. Mandate theory additionally legitimizes party influence and spells out some of the processes through which it works.

It is an important aspect of our models' success that they go against Marxist or Marxian critiques of "bourgeois democracy" as a sham, where electoral endorsements and party governments serve to mask the predominance of a socioeconomic or power elite. So far as we can see, party government in the U.S. works largely as mandate theories say it should, that is, responsively to electoral endorsements of party policy emphases.

Notes

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1. Tests of statistical significance are used solely to enhance parsimony of interpretation. We recognize that with a universe of the 37 (yearly) observations, these tests, used carelessly, could raise the likelihood of a Type II error. Therefore, we have not trimmed from the analysis variables whose values failed to attain "significance" but have reported them fully. A lack of "significance" means merely that the estimate for that value is not very reliable and should be read with caution.

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COOPERATION BY DESIGN: LEADERSHIP, STRUCTURE, AND COLLECTIVE DILEMMAS

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We return to the analysis of cooperation among interdependent rational individuals. We emphasize the limited impact of iteration (or repeated play) and explore the possibility of an alternative: intervention by rational agents, whom we call leaders. We show that leadership is more significant for initiating cooperation than for sustaining it. In addition, we identify two features of organizations that are critical in determining a leader's ability to initiate and sustain cooperation by structuring the incentives of his followers: the leader's capabilities (information and strategy sets) and reward structure (payoff function).

We examine the role of leadership in "collective dilemmas"—situations where individually rational behavior can prevent the securing of socially rational outcomes (see Axelrod 1981, 1984; Hardin 1982; Olson 1977; Taylor 1987). As many scholars have noted, iteration and retaliatory strategies provide ways to resolve collective dilemmas. Others (Alchian and Demsetz 1972; Frohlich and Oppenheimer 1978; Holstrom 1982; Kreps 1984; Miller 1987, 1988; Popkin 1979; and Putterman 1986) suggest that leaders—players with control over the distribution of benefits generated by collective action—can achieve and enforce cooperation. We provide an analysis of leadership in collective dilemmas.

The following image captures the problem we are examining. A group of rational egoists faces an array of possible equilibria in an iterated dilemma game. Only one of these equilibria yields the full benefits attainable from cooperation; but the players cannot attain it behaving as rational egoists. They therefore designate one of their member as a leader and ask the leader to apportion and withhold benefits in such a way as to make it in their individual self-interest to behave cooperatively. The question is, Can the leader succeed in initiating and sustaining cooperation?[1]

We show that the impact of leadership in collective dilemmas depends on a leader's incentives and capabilities. One might think of these characteristics as features of an institution—powers or resources that accrue to whoever holds the position of leader. Alternatively, one can think of players choosing someone to lead them but also deciding what powers to give to that individual.

Some types of leaders, we show, are able to monitor individual follower strategies and to target sanctions against shirkers. Given an appropriate strategy and "reputation" (follower beliefs about the leaders' payoffs) these leaders can induce other players to begin the game cooperating and continue to do so. However, other types of leaders—those who can

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only monitor and sanction groups of players rather than individuals—are generally unable to use their powers to initiate and sustain mutually beneficial cooperation. Thus, a leader's success at "solving the dilemma" depends on the leader's "reputation" and on exogenous features of the game that the leader may not control.

These results underscore an important role for leadership and institutions: by providing targetable and appropriate incentives, they make possible the provision of public goods. As is well known, the production of public goods is bedeviled with inappropriate incentives. By separating consumption from production and by making consumption possibilities contingent on individual effort in production, institutions can provide organizers with the means to motivate self-interested individuals to participate in collective action.

The results also address questions concerning the creation of organizations. Thus, our analysis is relevant to scholars who seek to explain hierarchies in political organizations (Miller 1987; Moe 1984), as well as to scholars who explore the origins of hierarchy and authority relations in market-like settings (e.g., Alchian and Demsetz 1972; Kreps 1984; Williamson 1985). It is relevant, in short, to much of the recent work in the "new institutionalism."

The Problem of Cooperation

The problem of cooperation can be analyzed in the following form. We will specify two iterated games. In the first, the *follower game*, three players, or followers, can engage in mutually beneficial but costly collective action. The second, *leader-follower game*, is the same except that a fourth player, the leader, controls the distribution of benefits produced by collective action. The results of this analysis are easily generalizable to the case of n followers acting by themselves or to the n follower–one leader case. (For an extended discussion of games of this type, see Bianco 1988.)

In the follower game, on each iteration t, follower i's strategy set contains two options: to cooperate ($s_{it} = 1$) or defect ($s_{it} = 0$). However, a follower who cooperates incurs the cost $c > 0$ that the one who defects does not incur. Cooperation also generates benefits that are distributed in equal shares to all followers. Given a vector of follower strategies s_t (one s_{it} per follower), the amount of benefits available for distribution equals $b(s_t)$. The relationship between follower strategies and benefits produced is specified as follows:

$$b(s_t) = a_1(s_{1t} + s_{2t} + s_{3t}) + a_2(s_{1t} \times s_{2t} + s_{1t} \times s_{3t} + s_{2t} \times s_{3t}). \quad (1)$$

When it is possible, we use β_n to refer to the quantity of benefits produced given an s_t in which n followers cooperate. Thus, given an s_t where two followers cooperate, $b(s_t) = \beta_2 = 2(a_1) + a_2$.

It should be noted and stressed that equation 1 is sufficiently general that it captures the characteristics of the major forms of collective dilemmas examined in the literature. For example,

1. Let $a_1 > 0$ and $a_2 = 0$. Then the amount of benefits is a linear function of the number of cooperators. This is the form of collective dilemma analyzed by Hardin (1971, 1982).
2. Let $a_1 \geq 0$ and $a_2 > 0$. Then the function yields team production, a canonical form of production externality that lies at the foundations of much of the new theory of the firm (Alchian and Demsetz 1972).

Either set of conditions yields the classical prisoner's dilemma (Axelrod 1981, 1984; Hardin 1971, 1982; Hardin and

Barry 1982). Given the centrality of the prisoner's dilemma to the literature on cooperation, it may be useful to illustrate the nature of the dilemma faced by the players in this follower game.

The players' payoffs are calculated as follows. A follower *i* who cooperates on iteration *t* receives the payoff $v_{it}(s_t) = b(s_t)/3 - c$, while a follower *i* who defects on iteration *t* receives the payoff $v_{it}(s_t) = b(s_t)/3$. For example, suppose $a_1 = 2$, $a_2 = 0$, and $c = 1$. Given that all other followers cooperate, follower *i* receives $(2 \times 3)/3 - 1 = 1$ for cooperating and $(2 \times 2)/3 = 1.33$ for defecting. Similarly, in a game where $a_2 = 2$, $a_1 = 1$, $c = 1.5$ and other players cooperate, follower *i* receives $[(2 \times 3) + (1 \times 3)]/3 - 1.5 = 1.5$ for cooperating and $[(2 \times 2) + (1 \times 1)]/3 = 1.67$ for defecting.

The dilemma of collective action arises when mutual cooperation makes all followers better off but each follower possesses a dominant strategy of defection at each iteration of the game. As shown previously, β_n is the amount of benefits produced on iteration *t* when exactly *n* players cooperate. In this case, a dilemma arises in a collective action game with $a_1 > 0$ and $a_2 = 0$ if and only if

$$\beta_2/3 > c > a_1/3. \tag{2}$$

If $\beta_2/3 > c$, all followers prefer any outcome where two or more followers cooperate to the outcome where all followers defect.[2] But if $c > a_1/3$, each follower prefers to defect regardless of what other followers do.

Table 1 gives an example of a collective action game satisfying relationship 2: $a_1 = 2$, $a_2 = 0$, and $c = 1$. Consider the leftmost column on Table 1. Assuming that both other followers cooperate, follower *i* receives $(3 \times 2)/3 - 1 = 1$ for cooperating and $(2 \times 2)/3 = 1.66$ for defecting. Therefore, *i* is better off defecting, *regardless* what other players do. The same is true if either one or none of follower *i*'s opponents cooperate. However, all followers receive a higher payoff in the two-cooperator outcome (.33) compared with the all-defect outcome (0). Hence, the dilemma.

Table 1. Follower i's Payoff in a Game Where $a_1 = 2$, $a_2 = 0$, and $c = 1$

	Strategies of other followers:		
	Both followers cooperate	One follower cooperates, one defects	Both followers defect
Cooperate $s_{it}=1$	1	.33	-.33
Defect $s_{it}=0$	1.11	.66	0

The same dilemma arises in a team production game when

$$\beta_2/3 > c > [\beta_3 - \beta_2]/3. \tag{3}$$

In words, $\beta_2/3 > c$ implies that all followers prefer an outcome where two or more followers cooperate to the all-defect outcome where no one cooperates. However, $c > [\beta_3 - \beta_2]/3$ implies that defection is each follower's dominant strategy. The payoff matrix for a team production game satisfying relationship 3—where $a_1 = 2$, $a_2 = 1$, and $c = 1.5$—is given in Table 2. Again, follower *i* is better off defecting, regardless what other players

Table 2. Follower i's Payoff in a Game Where $a_1 = 2$, $a_2 = 1$, and $c = 1.5$

	Strategies of other followers:		
	Both followers cooperate	One follower cooperates, one defects	Both followers defect
Cooperate $s_{it}=1$	1.5	.16	-.16
Defect $s_{it}=0$	1.66	.66	0

do. However, all followers prefer an outcome where two or more followers cooperate to the all-defect outcome.

A game subject to the constraint defined in equation 1 thus possesses the characteristics of a collective dilemma. In the classic literature on such dilemmas (esp. the prisoners' dilemma) iteration is offered as a way of resolving the problem of cooperation. In the section that follows, we argue that iteration is not enough.

The Necessity for Leadership: Why Iteration Is Not Enough

Robert Axelrod (1981, 1984) demonstrated to political scientists that in situations of repeated play, actors in prisoner's dilemmas might find it to their advantage to choose to cooperate. In more general form, the result is known as the Folk Theorem (see Fudenberg and Maskin 1986 for a review). It has not been sufficiently recognized that the implications of the Folk Theorem are not as optimistic as Axelrod's analysis suggests.

In the iterated version of the follower game, play continues for an infinite number of iterations, beginning with iteration 0. Each follower i will choose a strategy s_i, which gives i's strategy choice on iteration t, s_{it}, as a function of the history of the game, that is, as a function of other follower's strategy choices or of the amount of benefits produced on previous iterations. Follower's payoffs on iterations $t > 0$ are discounted by w^t, where $0 < w < 1$.

The Good News

According to the Folk Theorem, if the followers' discount rates are "high enough," full cooperation can be enforced as a subgame-perfect equilibrium (Selten 1975).[3] The followers need but employ trigger strategies (Friedman 1971; 1986, 85–104), with each follower cooperating until someone defects. The Folk Theorem suggests a focus on the trigger strategy that inflicts the largest possible punishment on a defector—if that threat does not deter defection, no other threat can. This strategy in the follower game will be labeled as "q-trigger" strategy (q for *quantity of benefits*):

q-trigger: $t = 0$: Cooperate.
$t > 0$: Cooperate if $b(s_{t^*}) = \beta_3$ for all $t^* < t$, defect otherwise.

A follower i using q-trigger cooperates as long as everyone else does, but any deviation from cooperation by another follower j triggers "permanent retaliation"—follower i refuses to cooperate on all subsequent iterations.

Contemporary theory indicates that the q-trigger strategy (or any trigger strategy) must be subgame-perfect to sustain cooperation in an iterated game (for accessible discussions of this point, see Bianco 1988; Friedman 1986, pp. 77–82, and 88–92; Ordeshook 1986, 137–42). To say that a situation where followers use the q-trigger strategy is a subgame perfect equilibrium implies two things. First, the follower's threats against would-be defectors are *effective*; a player cannot improve the payoff by deviating from cooperation, given that this deviation "triggers" punishment. Second, follower threats are *credible*; the followers are willing to carry out their threatened punishments if anyone defects.

Proposition 1 (proof in Appendix) gives the Folk Theorem result for the follower game.

PROPOSITION 1. *In a game where followers use the q-trigger strategy, full cooperation can be sustained as a subgame-perfect equilibrium if and only if* $w \geq 3c/\beta_2 - (\beta_3 - \beta_2)/\beta_2$.

The result is what one might expect, given the work of Axelrod (1981, 1984) and others: cooperation can be sustained as an equilibrium in a game involving repeated play. For example, in the iterated version of the game described in Table 1, the follower's use of the q-trigger strategy will yield full cooperation as an equilibrium outcome if $w \geq .25$. In an iteration version of the Table 2 game, cooperation can be sustained by the q-trigger strategy if $w \geq .1$.

The Bad News

The problem is that the Folk Theorem also states that *any* individually rational outcome can be supported as a subgame-perfect equilibrium, given an appropriate discount rate (Fudenberg and Maskin 1986). Moreover, full defection is a subgame-perfect equilibrium *regardless* of the discount rate.

There is thus a multiplicity of subgame-perfect equilibria in the follower game (and iterated dilemmas in general); full cooperation is but one of them. The problem of securing cooperation, then, is not one of creating incentives for players to cooperate when others are not doing so; nor is it one of *sustaining* cooperation. Rather, the problem of securing cooperation is one of getting to cooperation in the first place, of *attaining* cooperation.

Axelrod and others introduce the mechanism of evolution to resolve this problem (Axelrod 1981, 1984). Cooperation emerges in repeated play because the cooperators displace the noncooperators over time. However, this approach requires players to engage in nonequilibrium (i.e., nonoptimal) behavior. Alternatively, Axelrod posits a major role for norms, showing that societies in which opportunistic behavior is punished become societies inhabited by cooperators (Axelrod 1986). The problem with this argument is that the solution to the problem of cooperation remains exogenous. It is imposed, rather than contrived by those entrapped by the dilemma. We explore the feasibility of an alternative mechanism, that of human design.

Adding a Leader

We now examine the capacity of particular actors, whom we call leaders, to organize incentives to induce followers in a collective dilemma game to move toward mutually beneficial outcomes. In other words, we look at how adding an element of organization to decentralized behavior by interdependent actors might enable them to resolve the problem that repeated play does not resolve, the initiation of cooperation (as opposed to its maintenance).

We now turn to the leader-follower game, in which a new player, the leader (denoted as player 4), controls the distribution of the benefits produced by collective action. In analyzing the impact of leadership, we distinguish between different types of leaders. Leaders can vary in their capabilities and incentives, and these differences matter.

Leader Capabilities

By a leader's capabilities we mean a leader's strategy set and information about the strategies of followers. We distinguish between two levels of such capabilities: enhanced and limited.

An enhanced leader observes each follower's strategy choice s_{it} on each iteration t and can reward or punish each follower separately.[4] Given these capabilities, an enhanced leader is able to "target" rewards and punishments; that is, the leader can give followers the incentive to cooperate by distributing benefits to cooperators and withholding benefits from defectors. A trigger strategy that specifies this behavior will be denoted as "*e*-trigger" (*e* for *enhanced*). Formally,

e-trigger: $t = 0$: Reward follower i.
$t > 0$: Reward follower i if $s_{it^*} = 1$ on all $t^* < t$, punish follower i otherwise.

Thus, an enhanced leader can reward or punish a follower i regardless of what the leader does to another follower j.

Leaders can also be limited in their capabilities. A limited leader observes only aggregate output, $b(s_t)$, on each iteration t but is unable to discern the followers' individual strategy choices, s_{it}.[5] As a consequence, a limited leader possesses a limited strategy set: on an iteration t, such a leader can either reward or punish the followers but only as a group. In other words, the limited leader can use a trigger strategy, but only against all followers simultaneously. This strategy will be labeled "*l*-trigger" (*l* for *limited*).

l-trigger: $t = 0$: Reward all followers.
$t > 0$: Reward all followers if $b(s_{t^*}) = \beta_3$ for all $t^* < t$, punish all followers otherwise.

Leader Payoffs

The second critical feature is the nature of the leader's payoff function. We investigate two possibilities.

First, the leader could be a residual claimant. A leader who is a residual claimant is compensated for the costs of monitoring and supervision by receiving an initial share of benefits plus all the benefits not distributed to the followers.

In the context of our game, we assume that a residual claimant receives a share equal to σ $(1 > \sigma > 0)$ of each unit of benefits plus all the benefits not distributed to the followers. This characterization produces the following payoff function for a residual claimant leader:

$v_{4t}(s_t) = \sigma b(s_t)$ if leader rewards all followers
$= \sigma b(s_t) + 1/3(1 - \sigma)b(s_t)$ if leader rewards two followers and sanctions one follower
$= \sigma b(s_t) + 2/3(1 - \sigma)b(s_t)$ if leader rewards one follower and sanctions two followers
$= b(s_t)$ if leader sanctions all followers.

Leaders can also be motivated by being paid fixed shares; that is, they would receive a fixed share of the benefits from cooperation but not retain any benefits that they fail to distribute. As with a residual claimant, a fixed-share leader still has an incentive to motivate followers to cooperate: the leader's payoff increases with output. Here we will operationalize this form of leader payoff by assuming the leader always receives the share σ of the benefits produced. In this case, a fixed-share leader receives the payoff $v_{4t}(s_t) = \sigma b(s_t)$, given s_t, regardless of the leader's strategy choice on iteration t.[6]

Motivating leaders by making them residual claimants is a common feature in economic organizations. Alchian and Demsetz (1972) give a classic exposition of the argument. Other examples are found in the recent literature on the firm (Putterman 1986). Similar incentives are imputed to the managers of public bureaucracies, who are held by some to maximize the discretionary portions of their budgets (Bendor, Taylor, and Van Gaalen 1987; Niskanen 1971), that is, the portion over and above the actual costs of public programs.

Examples of leaders motivated by fixed shares would be managers and workers who are paid through profit sharing. In politics, examples would include party bosses (Erie 1988) or committee chairs (Arnold 1979, Ferejohn 1974) who exact a portion of the benefits resulting from collective action, be it the formation of electoral coalitions at the polls or legislative coalitions in the Congress.

The Impact of a Leader: Follower Strategies and Payoffs

We begin by noting how the addition of a leader affects the payoffs and strategies of the followers. The payoff function for follower i on an iteration t of the game becomes

$$\begin{aligned} v_{it}(s_t) &= (1 - \sigma)b(s_t)/3 - c \text{ if follower cooperates and is rewarded} \\ &= (1 - \sigma)b(s_t)/3 \text{ if follower defects and is rewarded} \\ &= -c \text{ if follower cooperates and is punished} \\ &= 0 \text{ if follower defects and is punished.} \end{aligned}$$

As in the follower game, follower i incurs a cost (c) in the leader-follower game whenever i cooperates. However, follower i receives the benefits of the group's efforts only if the leader chooses to reward i. Follower i receives exactly $(1 - \sigma)b(s_t)/3$ units of benefits when the leader rewards i—one-third of the benefits produced, minus the leader's share and any benefits the leader receives as a residual claimant.

It should be noted and stressed that the followers' payoffs under full cooperation with a leader are lower than their payoffs when they achieve cooperation by themselves. In the former case, follower i receives $1/3(1 - \sigma)b(s_t)$ units of benefits, while in the latter i receives $1/3b(s_t)$ units. This reduction in benefits arises because some benefits are used to compensate the leader.

Finally, adding a leader generates implications for the follower's trigger strategies. Without a leader, a follower using the q-trigger strategy cooperates until some other follower defects. However, the leader is now the source of rewards and punishments. In this case, the followers' strategies must be modified to contain threats that deter the leader from making unprovoked punishments. Such threats will be characterized in two ways.

By the first strategy, a follower retaliates if any other follower defects or if the leader fails to give rewards. This strategy is denoted "b-trigger" (b for *both*):

b-trigger: $t = 0$: Cooperate.
$t > 0$: Cooperate if $b(s_{t^*}) = \beta_3$ and the leader rewards follower i on all $t^* < t$, defect otherwise.

A follower i using the b-trigger strategy begins the game cooperating, cooperates as long as the leader rewards i and no follower defects but defects if the leader punishes i or if some follower defects.

Under a second trigger strategy, s-trigger (s for *strategy of leader*), a follower retaliates if the leader fails to reward the follower or if the follower's cost of cooperating exceeds the benefits. Formally,

s-trigger: $t = 0$: Cooperate.
$t > 0$: Cooperate if $b(s_{t^*}) \geq \beta_2$ and the leader rewards follower i on all $t^* < t$, defect otherwise.

We now proceed to the central part of our analysis—the conditions under which cooperation can be sustained and initiated in the leader-follower game.

Sustaining Cooperation with a Leader

We need to characterize the conditions under which cooperation can be sustained in the leader-follower game. To sustain cooperation, the threats of both the leaders and the followers must be effective and credible. Only in this way will cooperation be realized as a subgame-perfect equilibrium.

Characterization of the conditions under which cooperation can be sustained in the leader-follower game produces some surprising results. First, the conditions under which cooperation can be sustained with a leader are always narrower than the conditions in a game without a leader (and sometimes knife-edge or nonexistent). Second, these conditions vary with a leader's capabilities and reward structure.

Leaders

Suppose enhanced leaders use the *e*-trigger strategy and limited leaders use the *l*-trigger strategy. Proposition 2 (proof in Appendix) gives the condition under which the leader's retaliatory threats against other followers are effective:

PROPOSITION 2. *A leader's trigger strategy specifies effective threats against defection by follower i if and only if* $w \geq 3c/(1 - \sigma)\beta_2 - (\beta_3 - \beta_2)/\beta_2$.

As shown in the Appendix, the effectiveness condition is generated by comparing a follower's payoff under two scenarios: one where the follower plays a trigger strategy (and thus cooperates and is rewarded) and one where the follower switches to an all-defect strategy on iteration *t*, with retaliation by the leader commencing on iteration $t + 1$. As the value of *w* increases, the follower's payoff in the first scenario increases, while the payoff in the second remains the same. Thus, leader threats against a follower are effective if the value of *w* is "high enough." Both leader trigger strategies have the same effectiveness condition because they specify the same retaliation against defectors.

Proposition 3 (proof in Appendix) gives the conditions under which a leader's retaliatory threats are credible:

PROPOSITION 3. *The leader's trigger strategy specifies a credible threat against follower i if and only if a, b, or c is true:*

a. the leader has enhanced capabilities
b. the leader has limited capabilities, is a residual claimant, and $w \geq (1 - \sigma)$
c. other followers use the b-trigger strategy.

The significance of Proposition 3 is that the credibility of a leader's retaliatory threats depends on the leader's capabilities and incentives *and* on the strategy used by the followers. Under some conditions, certain leaders will have no incentive to commence retaliation against followers who defect. Such leaders cannot use trigger strategies to sustain cooperation, regardless of the severity of the punishments they could inflict on followers.

The problem of noncredible leader threats arises when the leader's retaliation against a defector "triggers" the remaining followers to switch from cooperation to defection. Obviously, this problem does not arise if followers use the *b*-trigger strategy (and cease to cooperate once someone defects) or if the leader has enhanced capabilities (and can retaliate against one follower while continuing to reward others).

However, consider a leader with limited capabilities, who must punish all followers simultaneously. If the followers use the *s*-trigger strategy, retaliation against a defector will cost a limited leader all future benefits from the remaining followers. Limited leaders who are residual claimants benefit on the initial iteration when they retaliate against a defector, so their threats may be credible. However, fixed-share leaders, whose single-iteration payoff is the same regardless whether they reward or punish followers, need enhanced capabilities in order to have credible threats.

Table 3. Conditions for Sustaining Cooperation, Given Leader Type and Follower Strategies

		Followers' Strategy	
		s-trigger	b-trigger
Enhanced Leader	Residual Claimant	$w \geq \max(w_1, w_2)$	
	Fixed Share	$w \geq w_1$	
Limited Leader	Residual Claimant	$w_2 = w \geq w$	$w \geq \max(w_1, w_2)$
	Fixed Share	Never	$w \geq w_1$

$w_1 = 3c/(1-\sigma)\beta_2 - (\beta_3-\beta_2)/\beta_2$
$w_2 = (1-\sigma)$

Followers

Proposition 4 gives the condition under which the followers possess an effective threat against a leader's switch to unprovoked punishment.

PROPOSITION 4. *The followers' trigger strategies (b-trigger or s-trigger) specify effective threats against unprovoked punishment by the leader if and only if a or b is true:*

a. the leader is a residual claimant and $w \geq (1 - \sigma)$

b. the leader receives a fixed share of output.

Again, effectiveness requires that the leader's loss from retaliation equal or exceed the leader's gain from unprovoked punishment. If the leader is a residual claimant, this requirement is met if w is "high enough." Threats against a fixed-share leader are always effective because such a leader does not increase the single-iteration payoff by defecting.

The final step is to specify the conditions under which follower threats against the leader are credible. The following proposition is presented without proof:

PROPOSITION 5. *Follower threats against unprovoked punishments by the leader (as specified by b-trigger or s-trigger) are always credible.*

The intuition behind Proposition 5 is simple: once a leader commences unprovoked punishment of the followers, followers are willing to retaliate because it saves them the cost of cooperation without triggering any reduction in their benefits.

We summarize the information contained in Propositions 2–5 in Table 3, in which we give the conditions for sustaining cooperation in a leader-follower game as a function of leader type (payoff structure and capabilities) and follower strategies.[7] For a specific leader type and follower strategy, each cell in Table 3 records the condition under which the leader has an effective, credible threat against the followers and the followers possess an effective, credible threat against the leader.

The most significant finding contained in Table 3 is that irrespective of the capabilities of the leader or the strategies of the followers, the conditions under which cooperation can be sustained with a leader are narrower than the same conditions in the absence of a leader. In each cell of Table 3, a necessary condition to sustain cooperation is that

$$w \geq w_1 = 3c/(1 - \sigma)\beta_2 - [\beta_3 - \beta_2]/\beta_2.$$

However, Proposition 1 shows that cooperation can be sustained in a game without a leader if $w \geq 3c/\beta_2 - [\beta_3 - \beta_2]/\beta_2$. Thus, since $1 > \sigma > 0$, adding a leader increases the value of w needed to sustain cooperation. The reason is that some of the benefits of collective action in the leader-follower game are used to compensate the leader. Therefore, the threat of losing all future benefits is a less severe punishment for a would-be defector in the leader-follower game, as compared with the same punishment in the follower game. Since threats in the leader-follower game are less severe, a higher discount rate is required to sustain cooperation.

In addition, Table 3 shows that certain combinations of leader type and follower strategies makes it impossible to sustain cooperation (limited-fixed-share leader, followers use *s*-trigger) or possible only in knife-edge circumstances (residual claimant leader with limited capabilities, followers use *s*-trigger).

In general, adding a leader makes cooperation harder to sustain, not easier. By implication, the benefits of having a leader must lie in the leader's ability to help followers initiate, not sustain, cooperation. We now assess this claim.

The Leader's Role in Initiating Cooperation

To initiate cooperation in a collective dilemma, the leader must somehow give each follower an incentive to cooperate on the *first* iteration of the game, regardless what other followers do. Of course, the follower will prefer to defect if the leader does not have a trigger strategy that generates effective, credible threats against defection. Suppose this requirement is met. In that case, each follower is better off cooperating on the first iteration (and thereafter) if the follower believes that the leader will actually use the trigger strategy when the game is played. That is, the trigger strategy must be the leader's dominant strategy.

As we show here, the first barrier to a leader's success at initiating cooperation is that regardless of leader type, under complete information the trigger strategy is never a dominant strategy. Thus, under complete information a leader cannot use rewards and punishments to initiate cooperation. Of course, as Miller (1987) argues, by transforming the leader-follower game into a game of incomplete information, it is possible to generate follower beliefs (Kreps and Wilson 1982a, 1982b) that a trigger strategy is dominant for the leader regardless of the "true" configuration of leader payoffs. We show here, however, that even the strongest of leader reputations is not sufficient to initiate cooperation. Other things, such as appropriate leader capabilities, are required.

The Dominance of Trigger Strategies

As Proposition 6 shows (proof omitted), under complete information trigger strategies are never dominant strategies for a leader.

PROPOSITION 6. *Regardless of a leader's capabilities and reward structure, a trigger strategy is never the leader's dominant strategy in the leader-follower game.*

This result confirms the conjecture of Miller (1987, 1988) that apart from the effects of reputation, a leader's promise to reward and punish according to a trigger strategy is not credible. Put simply, it is always possible to devise a follower strategy such that the leader prefers to deviate from the trigger strategy.[8] For example, suppose followers use the following strategy:

$t = 0$: Defect.

$t > 0$: Cooperate if the leader punished on iteration 0, defect otherwise.

If followers use this strategy, a leader (regardless of type) receives a higher payoff by punishing on the first iteration and rewarding thereafter than by using a trigger strategy (which will trigger defection by the followers beginning on iteration 1). The trigger strategy cannot, therefore, be a dominant strategy for the leader.

Some types of leaders will prefer to deviate from their trigger strategies even when followers use trigger strategies or the all-defect strategy. If a residual claimant leader expects a follower to defect on iteration t (including $t = 0$), the leader can improve the payoff by punishing the defector on iteration t instead of waiting to commence punishment on iteration $t + 1$ as the trigger strategy specifies. If followers use the *b*-trigger strategy, residual claimant leaders with enhanced capabilities can improve their payoff, given that one follower defects, by deviating from the *e*-trigger strategy and punishing all followers after the defection. In both cases, the leader's incentive to deviate from the trigger strategy implies that the strategy is not the dominant strategy.

Fixed-share leaders face similar problems. A fixed-share leader with limited capabilities is not willing to retaliate against a defector unless other followers use the *b*-trigger strategy. This result is detailed in Proposition 3. Again, the implication is clear: the leader's trigger strategy is not the dominant strategy.

The point is simple: under complete information, even if leaders and followers possess effective, credible retaliatory threats, a leader's trigger strategy is never the dominant strategy. No type of leader is willing to reward followers initially, continue to reward cooperation, and punish defection regardless of the follower's strategies and other parameters in the game. Thus, under complete information leaders face the same problem as followers in a game without a leader—they are willing to use a trigger strategy, but only if everyone else does. Equivalently, the Folk Theorem has the same implications for the leader-follower game as for the follower game: given a high enough discount rate, there are infinitely many equilibria—including full cooperation and full defection. Adding a leader does not reduce the number of equilibria or drive followers to the full cooperation outcome.

Adding Reputations

Suppose the followers are *certain* (because of their beliefs about the leader's payoffs) that the leader will reward and punish according to a trigger strategy (*l*-trigger for a limited leader, *e*-trigger for an enhanced leader). Assume, further, that the leader's threats are effective; this, along with the follower's beliefs, ensures that the leader's trigger strategy will sustain cooperation. Then, to initiate cooperation, given followers' beliefs, a strategy which specifies cooperation initially and as long as the leader rewards must maximize the payoff of each follower. (Here we focus on the strategies *b*-trigger and *s*-trigger.) If some other strategy yields a higher payoff for the follower in some situation, reputation effects will not serve to initiate cooperation.

This argument provides a simple test of the ability of reputation to induce cooperation. Proposition 7 (proof in Appendix) summarizes the result of this test:

PROPOSITION 7. *If followers are certain that the leader will use the strategy l-trigger if the leader has limited capabilities and e-trigger if the leader has enhanced capabilities and in addition* $w \geq w_1$, then

a. *if the leader has enhanced capabilities, b-trigger or s-trigger maximizes a follower's payoff*

b. *if the leader has limited capabilities, a follower's payoff-maximizing strategy depends on the strategies used by other followers.*

Thus, the follower's response to a leader's "reputation" of rewarding cooperation and punishing defection depends on the leader's capabilities. If the leader possesses enhanced capabilities, the follower is best off cooperating on the first iteration and thereafter—given beliefs, the follower expects this behavior to be rewarded regardless of what the other followers do. However, if the leader has limited capabilities, a follower will be punished if *anyone* defects. Thus, the follower is willing to cooperate initially only if everyone else does so. In summary, reputations and effective threats serve to initiate and sustain cooperation, given enhanced leaders but not given leaders with limited capabilities.

Conclusion

Employing a game that highlights the dilemmas confronting interdependent rational individuals, we exposed the limited impact of iteration (or repeated play) and explored the possibility of an alternative —rational intervention by a third party, whom we have called a leader.

Our analysis has shown that

1. In collective dilemmas, leadership is more significant for initiating cooperation than for sustaining it.
2. Leaders must be differentiated in terms of capabilities (information and strategy sets) and incentives (payoff functions).
3. Variation in the capabilities and reward structures for leaders strongly condition their ability to sustain and to initiate cooperation in collective dilemmas.

Rather than positing an enlightened leader who intervenes in a welfare-enhancing manner, we have instead made the behavior of the leader endogenous. An important feature of our analysis is that it offers insight into the impact of organization. The features of the organization that structure the choices of leaders are the system of rewards and the information and incentives at the leader's command. Our analysis suggests that proponents of cooperation would be wise to endow their leader with enhanced capabilities. Given effective threats and an appropriate reputation, a leader with enhanced capabilities can use a trigger strategy to initiate and sustain cooperative behavior by followers. The effect of a leader's reward structure on the ability to initiate cooperation is less clear. Regardless of how a leader is compensated, it is possible to construct follower beliefs about leader payoffs such that the followers will expect the leader to reward and punish according to a trigger strategy when the game is played. However, rewarding a leader with a fixed share appears to simplify the problem of constructing a reputation that convinces followers that the leader will use a trigger strategy. Fixed-share leaders appear willing to reward followers initially and on subsequent iterations unless the rewards induce follower defections. In contrast, even if rewards do not induce defections, residual claimant leaders may have an incentive to punish followers initially or to punish spontaneously on subsequent iterations.

Appendix: Proofs of Propositions

Proof of Proposition 1. Suppose all followers use the q-trigger strategy. In this case, follower i's payoff for the multiple iteration game equals $(\beta_3/3 - c)/(1 - w)$. If follower i makes the payoff-maximizing deviation from full cooperation (i.e., to full defection) and other followers respond by ceasing to cooperate, i's payoff for the whole game equals $\beta_2/3$. Note that each follower j's ($j \neq i$) response to follower i's defection is optimal, given that all followers so respond. In this case, follower i lacks incentive to switch strategies if and only if $(\beta_3/3 - c)/(1 - w) \geq \beta_2/3$ or $w \geq 3c/\beta_2 - (\beta_3 - \beta_2)/\beta_2$. Thus, the vector where all followers use the q-trigger strategy is subgame-perfect if and only if this condition holds. QED

Proof of Proposition 2. Consider a follower i. If i (along with the other followers) uses a trigger strategy, i's payoff for the entire game equals $[(1 - \sigma)\beta_3/3 - c]/(1 - w)$. If follower i defects and the leader retaliates, follower i's payoff equals $(1 - \sigma)\beta_2/3$. Thus, the leader's threat is effective if and only if $[(1 - \sigma)\beta_3/3 - c]/(1 - w) \geq (1 - \sigma)\beta_2$ or $w \geq 3c/(1 - \sigma)\beta_2 - (\beta_3 - \beta_2)/\beta_2$. QED

Proof of Proposition 3. Consider a game where a follower i switches to full defection beginning on iteration t. The leader's threat (regardless of whether the leader uses l-trigger or e-trigger) is to cease rewarding follower i beginning on iteration $t + 1$.

If the remaining followers use b-trigger, the leader's threat is always credible: no follower will cooperate after follower i defects, thus the leader does not lose any future benefits from punishing follower i.

The situation is different when the remaining followers use s-trigger. A leader with enhanced capabilities can punish follower i without provoking any other followers to defect, thus the leader's threat is credible.

If the leader has limited capabilities and followers use s-trigger, retaliation against follower i provokes the other followers to cease cooperating beginning on iteration $t + 2$. If the leader is also a residual claimant, the leader's payoff from iteration $t + 1$ forward, given that the leader uses l-trigger (and punishes everyone beginning on $t + 1$) equals β_2, while the payoff from not retaliating equals $\sigma\beta_2/(1 - w)$. Thus, the payoff from retaliating equals or exceeds the payoff from not retaliating (and thus the threat is credible) if and only if $\beta_2 \geq \sigma\beta_2/(1 - w)$ or $(1 - \sigma) \leq w$. Finally, if the limited leader receives a fixed share and followers use s-trigger, the payoff from retaliating equals $\sigma\beta_2$ and from not retaliating equals $\sigma\beta_2/(1 - w)$. Since this leader's payoff from retaliation is always lower, the threat is never credible.

In summary, the leader always has a credible threat if followers use b-trigger; if the leader has enhanced capabilities; or if the leader has limited capabilities, is a residual claimant, and $w \leq (1 - \sigma)$. The leader's threat is never credible when the leader has limited capabilities and receives a fixed share of output and followers use the s-trigger strategy. QED

Proof of Proposition 4. Suppose a leader contemplates switching to the payoff-maximizing defection strategy (all-defect) on iteration t.

If the leader is a residual claimant, the payoff from t forward, given that the leader switches and followers retaliate on $t + 1$ (follower retaliation is the same regardless of whether they use b-trigger or s-trigger), equals $\beta_3 + 0$. However, if the leader continues to use a trigger strategy, the payoff from iteration t forward equals $\sigma\beta_3/(1 - w)$. Thus, the follower's threat is effective if $\sigma\beta_3/(1 - w) \geq \beta_3$ or $w \geq (1 - \sigma)$.

If the leader receives a fixed share, the payoff from all-defect equals $\sigma\beta_3$, while the payoff from using a trigger strategy equals $\sigma\beta_3/(1 - w)$. Thus, the follower's threat is always effective in this case. QED

Proof of Proposition 7. If the leader is enhanced and uses e-trigger, follower i will be rewarded until i defects. By Proposition 2, follower i lacks incentive to switch from b-trigger or s-trigger to any defection strategy if and only if $w \leq 3c/(1 - \sigma)\beta_2 - (\beta_3 - \beta_2)/\beta_2 = w_1$. Thus, either trigger strategy maximizes follower i's payoff.

If the leader is limited and uses l-trigger, follower i again has no incentive to provoke retaliation by being the first to defect, given $w \geq w_1$. However, if some other follower defects on iteration t, follower i (along with everyone else) will be punished beginning on iteration $t + 1$. In this case, follower i is better off defecting on iteration t rather than using the trigger strategy (defecting on iteration $t + 2$ and after for s-trigger and on $t + 1$ and after for b-trigger). Thus, a trigger strategy maximizes follower i's payoff, given a limited leader and $w \geq w_1$ only if all other followers use trigger strategies. However, if some follower uses a strategy that specifies defection beginning on iteration t, follower i maximizes the payoff by using the same strategy. QED

Notes

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1. We focus on the question of whether control over benefits gives the leader any ability to initiate and sustain cooperation in iterated dilemma games. Our analysis does not consider the role of leaders in games that model other situations of cooperation under anarchy, such as chicken, stag hunt, battle of the sexes, etc. For insightful discussions of these games, see Oye 1985 and Schelling 1960. Our analysis also ignores the fact that leaders may also help to achieve cooperation by helping followers coordinate their actions or by persuading followers to change their feelings and beliefs. For discussions of coordination, see Banks and Calvert 1989 and Calvert 1989. The cognitive and emotional aspects of leadership are investigated more fully in the sociology and psychology literature and remain a challenge to rational choice analyses of leadership.

2. A weaker condition is possible: the "dilemma" would still arise if all followers (rather than two) had to cooperate to produce an outcome preferable to all-defect. The condition specified here allows examination of certain follower-leader interactions, which will be described.

3. We focus on the follower's (and later the leader's) ability to enforce full cooperation with retaliatory threats. There are three reasons to focus on full cooperation: the outcome maximizes the payoffs of cooperation, maximizes the severity of threats against would-be defectors, and is arguably a "Schelling point" (Schelling 1960).

4. Such a leader is specified by Alchian and Demsetz (1974, 118) and Miller (1988, 13).

5. This type of leader is developed by Holmstrom (1982), and mentioned by Miller (1988). This construction is consistent with the assumption that the leader can in principle observe follower strategies but that it is prohibitively costly to do so.

6. This specification of a fixed-share leader *does not* satisfy the "budget balancing" condition given in Holmstrom 1982. Intuitively, if a fixed-share leader punishes the workers, all residual benefits above the leader's share are not distributed to anyone. As Holmstrom's critics point out (Eswaran and Kotwal 1984), if these benefits are given to an additional player (a "sponge"), this additional player may have an incentive to disrupt successful team production and thereby increase personal payoff. Here we focus on the effect of having a fixed-share leader without considering how undistributed benefits are allocated.

7. Consider the case where the leader is a limited residual claimant and the other players use the s-trigger strategy. The leader's threats are effective if and only if $w \geq w_1$ (Proposition 2) and credible if and only if $w \leq w_2$ (Proposition 3). Player threats against the leader are effective if and only if $w \geq w_2$ (Proposition 4), and always credible (Proposition 5). Putting these requirements together, the leader and the players have effective, credible threats if and only if $w_2 = w \geq w_1$.

A second example. Suppose the leader is an enhanced residual claimant and the other players use the b-trigger strategy. The leader's threats are effective if and only if $w \geq w_1$ (Proposition 2) and always credible (Proposition 3), while the players' threats are effective if and only if $w \geq w_2$ (Proposition 4) and always credible (Proposition 5). Thus, cooperation can be sustained in this case if and only if $w \geq \max(w_1, w_2)$.

8. We thank Randy Calvert, University of Rochester, for his help in clarifying this point.

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ARE CONGRESSIONAL COMMITTEES COMPOSED OF PREFERENCE OUTLIERS?

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A diverse set of congressional studies portrays members of standing committees as more or less homogeneous "high demanders" or "preference outliers" relative to members of the larger legislature. Using interest group ratings of members of the Ninety-sixth to Ninety-ninth Congresses, I conduct conventional statistical hypothesis tests to discern whether standing committees are more extreme and more homogeneous than the legislature as a whole. With only a few exceptions, the tests do not allow confident rejection of null hypotheses of identical committee and chamber preferences. The absence of convincing evidence of preference outliers is broadly consistent with emerging incomplete information game-theoretic legislative research and difficult to reconcile with many previous formal theories of legislative politics.

A congressional claim so common that it approaches conventional wisdom is that standing committees are powerful in obtaining disproportionate policy benefits within their jurisdictions. This thesis emerged from empirical studies of the nineteenth century (McConachie 1898; Wilson 1885), persisted through the postwar behavioral revolution (e.g., Fenno 1966, 1973; Manley 1970; Price 1978), and has survived into the 1980s (Shepsle and Weingast 1987). An integral part of many claims regarding committee power is that committee members' preferences differ systematically from those of the larger legislature. In particular, the belief is that committee members more or less homogeneously demand high levels of benefits from policies that fall into their committees' jurisdictions. Indeed, in recent formal models, divergence in committee-chamber preferences is one of several necessary conditions for committee power (e.g., Denzau and Mackay 1983; Shepsle and Weingast 1987; Weingast and Marshall 1987).

The dominant view among empirical and theoretical scholars is summarized in two quotations. Davidson offers a generalization based on his observation of congressional activity: "Under pressure from members and factions desiring representation, party leaders not only have allowed assignments to proliferate but have tended to accede to members' preferences for assignments. Inevitably, this means that legislators gravitate to those committees with which they, or their constituents, have the greatest affinity. Thus, many congressional workgroups are not microcosms of the parent houses, but are biased in one way or another" (1981, 111). Others use empirical generalizations such as Davidson's to defend a theoretical assumption that committees have policy biases. This approach, now common in the public choice literature, was first taken by Niskanen, who wrote that "[in this model the] committees for each serv-

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ice are dominated by representatives of the group with the *highest relative demand* for the service. (One might think it equally plausible that the committees would consist of those representatives who have the highest and lowest demands for a specific service. A characteristic of legislatures, however, is that *advocacy is concentrated and opposition is diluted.*)" (1971, 139, emphasis added). (See also Benson 1981, 1983; Mackay and Weaver 1979, 1983; Weingast and Marshall 1987; Weingast and Moran 1983). In spite of the plausibility of such observations and corresponding assumptions, the evidence for what have been called "preference outliers," "high-demand committees," "self-selection tendencies," and ultimately "committee power" is inconclusive. For instance, while Ray (1980) interprets his results as supportive of the claim that the Armed Services Committee has high demanders, the findings of Stephens (1971) and Goss (1972) are quite different. Arnold (1979) therefore analyzes the committee's representativeness over a twelve-Congress period and finds substantial variation over time as well as across measures. Similarly, Weingast and Marshall (1987) present results indicating that some committees are outliers; however, the authors do not attempt to reconcile observed differences in the direction of outliers, such as the preponderance of defense advocates on Armed Services but defense opponents on Foreign Affairs.

Unanswered questions also accompany findings such as Cowart's (1981) and Shepsle's (1978). Cowart claims support for the outlier hypothesis when interpreting constituency characteristics as policy-specific measures of demand. In a similar vein, Shepsle estimates several probit models in which freshmen's requests are predicted by constituency characteristics. Interpreting these as "demand equations," the observed relationship is the first part of a multistage causal argument. Because constituency characteristics are associated with requests, and requests in turn tend to be granted, the assignment process as a whole is governed by self-selection, yielding an "Interest-Advocacy-Accommodation Syndrome" (1987, 231). Munger (1988) expresses reservations about one stage of Shepsle's argument, pointing out that it is impossible to know how frequently legislators actually request their (or their constituents') most-preferred committees. Another stage is implicitly challenged by Jewell and Chi-hung's (1974) findings that freshmen typically do *not* receive their first choices and rarely receive prestigious assignments. Self-selection tendencies, therefore, may be overstated.

Others have questioned the generality of characterizations of congressional committees as homogeneous high demanders. Fiorina (1981), for example, points out that committee members may be extreme on both sides of their committee's policy spectrum. Davidson (1981), too, notes several exceptions to his generalization. Finally, the empirical results of Cook (1983), Fowler, Douglass, and Clark (1980) and Rivers and I (Krehbiel and Rivers 1988) raise deeper questions about whether assignment patterns are as important for legislators' electoral success and for committee power as much of the literature seems to presume. At the very least, a new empirical assessment seems worthwhile.

What Are Preference Outliers?

There are three types of preference outliers. First, a *classical homogeneous high-demand outlier*, as characterized in Niskanen's seminal book (1971), is a committee whose members have a common desire for uniquely high levels of benefits from policies within their committee's jurisdiction. Advocates of this view, such as Weingast and Moran (1983), assert that

Congress can be characterized as follows:

> Each legislator gives up some influence over many areas of policy in return for much greater influence over the one that, for him [sic], counts the most. Thus, we find that representatives from farming districts dominate agriculture committees and oversee the provision of benefits to their farm constituents. Members from urban districts dominate banking, urban, and welfare committees overseeing an array of programs that provide benefits to a host of urban constituents. And members from western states dominate interior and public lands committees that provide benefits to their constituents (pp. 771–72).

Conditions for convincing empirical support for this form of preference outliers are straightforward. Given estimates of the policy positions taken by legislators, committee members' positions should be systematically different from those of the entire legislature in two ways: central tendencies (means, medians) and dispersion of policy positions (variances). In other words, the classical view has both an *outlier component* and a *homogeneity component.*

Second, a *bipolar outlier* is a committee that has significant factions of members on both sides of its policy spectrum. For example, a judiciary committee may be composed exclusively of high demanders —ostensibly a la Niskanen (1971)—but with one faction of extreme demanders of civil rights and another faction of extreme demanders of so-called law-and-order measures. The probable policy consequences of this form of outlying committee are much different from those of the classical homogeneous high-demand type. So, too, are its directly observable manifestations. For bipolar outliers, we would expect not to see systematic differences in committee-chamber means or medians. But we would observe significant differences in committee-chamber variances, since committee members' positions are substantially more dispersed than those of the parent body.[1]

Third, an *intense interest* or *high-salience outlier* is a committee whose members share a uniquely high level of intrinsic interest in the committee, perhaps because its policy domain is highly salient to members' constituents. While this type of committee may be regarded as a preference outlier, it is for reasons quite apart from the policies espoused or level of services demanded by its members. Rather, self-selection in this context simply means following one's (constituents') innate policy interests to committees with compatible jurisdictions. This type captures the notion of intense preferences and obviously cannot be detected from means, medians, or variances. However, a relationship between this and other conceptions of preference outliers must be noted. If preference outliers of the first or second forms do not exist—that is, if the distribution of policy preferences within committees is essentially the same as that within the legislature—the existence of intense interest or high-salience outliers would be inconsequential for the ultimate distribution of policy benefits. Perhaps good public policy a la Fenno (1973) is more likely to emerge when committees are composed of members who have uniquely strong interests in their committee's work. However, if such members share the policy preferences of the larger legislature, the alleged committee power emerging from the division-of-labor arrangement cannot be a form of power based on disproportionate distributive benefits.

Table 1 summarizes the types of preference outliers and their observational attributes. The empirical analysis that follows focuses on the classical homogeneous high-demand type and its corresponding outlier and homogeneity components. Bipolar outliers can also be detected with available data and will be noted if and when observed, although this is a lesser interest. Finally, since high-salience or intense interest cannot be measured with the data and techniques em-

Table 1. Types and Observability of Preference Outliers

	Observation	
Type of Outlier[a]	Difference in Mean	Difference in Variance
Classical (homogeneous, high demanders)	yes ($\bar{x}_c > \bar{x}_h$)	yes ($s_h^2 > s_c^2$)
Bipolar (heterogeneous, extreme demanders)	no ($\bar{x}_c = \bar{x}_h$)	yes ($s_c^2 > s_h^2$)

[a] Intense-interest or high-salience outliers cannot be observed.

ployed, no inferences about this type will be made.

Data and Hypotheses

To assess the evidence for homogeneous high demanders in the contemporary Congress, I rely on the assessments of some of the closest observers of congressional activity: public and private interest groups.[2] These groups are:

American Conservative Union (ACU)
Americans for Democratic Action (ADA)
American Security Council (ASC)
Business-Industry Political Action Committee (BIPAC)
Chamber of Commerce of United States (CCUS)
Committee on Political Education (AFL-CIO) (COPE)
International Brotherhood of Teamsters (IBT)
League of Conservation Voters (LCV)
Machinists Nonpartisan Political League (MNPL)
National Council of Senior Citizens (NCSC)
National Educators Association (NEA)
National Farmers Union (NFU)
Public Citizens Congress Watch (PCCW)
Railway Labor Executives Association (RLEA)

While not immune from criticism (Fowler 1982), interest group ratings have several unique advantages for studying the composition of committees.

First, although the ratings are based on small samples of roll call votes and although voting is only one of several significant stages of the legislative *process*, the votes are nevertheless carefully selected by organizations that have strong *incentives to know the implications* of such processes for the legislative *product*. Consequently, the revealed preferences in these votes are likely to differentiate legislators according to genuine policy differences rather than inconsequential or symbolic behavior.

A second advantage of using interest group ratings pertains to agenda processes. Many roll call studies are indiscriminately based on very large samples of votes that are inherently determined by endogenous agenda formation processes. Neither these processes nor the systematic biases they induce in votes are well understood. In contrast, when selecting significant votes for the computation of published ratings, interest groups incorporate information about specific and sometimes arcane strategic settings. For example, inspection of the sets of votes used in this study reveals a nontrivial fraction of what a naive observer would regard as procedural questions, such as votes on special orders or on motions to instruct conferees. For some applications, inclusion of a large number of so-called procedural votes would make inferences hopelessly ambiguous. However, I not only acknowledge but also exploit the selection bias of informed congressional observers.

Third, the proliferation of interest group ratings in recent years provides an opportunity to assess committee-chamber differences with more precision than was previously possible via general party support scores, for example, since many contemporary interest group ratings are policy-specific and thus jurisdiction-specific. Accordingly, in addition to assessing the evidence for preference outliers from the standpoint of general ideology measures, this study looks closely at differences in committee-chamber preferences on issues that fall predominantly in the jurisdiction of the specific committees.

As discussed above, the classical preference outlier perspective has two components: an *outlier component,* which pertains to central tendencies of committee members as compared with noncommittee members, and a *homogeneity component,* which pertains to the degree to which committee members are in better agreement with themselves than are members of the larger legislature. Two types of tests will be conducted for each component.

To assess whether or which committees are composed of outliers, I compute and compare median and mean ratings of committees with those of the entire legislature. The focus on medians is justified by formal models that typically yield variations of median voter results. Support for the preference outlier claim requires that these medians be significantly different. One method of assessing significance is simply the percentage of legislators whose ratings lie between the committee and chamber medians. A second method is a standard difference-in-means test. The null hypothesis is H_1°: $\mu_h = \mu_c$, where μ_h is the average policy position of the house (excluding committee members) and μ_c is the comparable average for the committee. The alternative hypothesis is H_1^A: $\mu_h < \mu_c$. Rejection of the null hypothesis is necessary for support of the claim that committees are composed of outliers. A one-tailed *t*-test is used where, for example, rejection of the null hypothesis at the .05 significance level requires a *t*-statistic of 1.65 or greater.[3]

Analogously, to assess whether committees are significantly more homogeneous than the House, a median-based approximation is obtained by ascertaining whether, for any given committee, its Democratic and Republican median members are both on the same side of the House median. When this condition holds, the committee is said to be homogeneous, since majorities of committee members of *both* parties have ratings that exceed those of a majority of the House. Homogeneity of this sort facilitates reaching a bipartisan consensus on the committee. When this condition does not hold, the committee is said to be heterogeneous since committee members' preferences are sufficiently different from one another that a committee majority of only *one* party has ratings that exceed those of a majority of the House. Heterogeneity in this sense makes reaching a bipartisan consensus within committee difficult. A similar but more rigorous test of homogeneity focuses on the relative variance of the committee and the House. The null *hypothesis is* H_2°: $\sigma_h^2 = \sigma_c^2$, where σ_h^2 is the variance for the House (excluding committee members) and σ_c^2 is the variance for the committee. The alternative hypothesis is H_2^A: $s_h^2 > s_c^2$. Rejection of the null hypothesis is necessary for support for the homogeneity component of the classical view of committee composition. The ratio of the variances, σ_h^2/σ_c^2, has an *F*-distribution. Under the appropriate one-tailed test we reject the null only if the test statistic, s_h^2/s_c^2, is above the critical value given by F_{N_h,N_c}, where N_c is the number of members on the committee minus 1, and N_h is the number of members in the House minus N_c.[4]

The analysis is conducted in two stages. A first approximation permits inspection of all standing committees and focuses on general ideology ratings. More refined

Table 2. Preference Outliers Based on ADA Ratings

Standing Committees	Difference in Medians (1)	Percentage in Gap (2)	t-statistic for H_1° (3)	p-value for H_2° (4)
Ways and Means	2.0	.9	.46	.47
Public Works and Transportation	3.0	1.4	.23	.42
Agriculture	-3.0	2.8	-.89	.13
Veterans' Affairs	-3.5	3.0	-.70	.14
Government Operations	4.0	1.8	.52	.73
Budget	5.5	2.1	.38	.73
Energy and Commerce	5.5	2.1	.04	.62
Appropriations	7.0	3.0	1.01	.40
Merchant Marine and Fisheries	-7.0	4.1	-.45	.22
Small Business	-8.5	5.5	-.59	.47
Science and Technology	-9.0	6.0	-.91	.19
Interior	9.5	3.4	.41	.91
Banking, Finance, and Urban Affairs	13.0	6.0	.46	.52
Foreign Affairs	13.0	6.0	1.80	.74
Standards of Official Conduct	-14.0	9.4	-.30	.63
Judiciary	18.5	10.6	1.19	.85
Education and Labor	21.5	12.2	2.05	.44
Rules	22.0	12.6	1.20	.70
House Administration	23.0	13.3	.69	.67
Armed Services	-27.5	17.9	-3.72	.04
Post Office and Civil Service	35.5	25.7	2.20	.67
District of Columbia	50.5	37.9	1.96	.84

Note: House median = 44.50; mean = 47.32; standard deviation = 34.39. All party medians are heterogeneous except for the Armed Services Committee, for which the median is homogeneous.

tests are then conducted by analyzing policy-specific ratings. Due to the more precise information available at this stage (albeit for only a subset of committees), the alternative hypotheses face a better chance of corroboration. For example, we would expect committee-chamber differences to be greater with policy-specific measures than with the general ideology measures and especially pronounced on committees where the goal of the high demanders is acquisition of concentrated benefits, that is, distributive policies. Such committees in the House, for example, include Public Works, Interior, and Agriculture. The analysis reported in detail focuses on the House of Representatives in the Ninety-ninth Congress, while a subsequent section summarizes comparable results for the Ninety-sixth to Ninety-eighth Congresses, including the Senate.

General Ideology Results

The purpose of the first set of results is very narrow: to see whether systematic and general biases in general ideology outliers exist or whether cross-committee variation exists. Although these findings are clearly not sufficient for drawing any precise conclusions about preference outliers, they provide a useful baseline for subsequent results and their interpretation.

Ratings of the Americans for Democratic Action (ADA) and the American Conservative Union (ACU) were first analyzed for the House in the Ninety-ninth Congress. The results were virtually identical for the two sets of ratings, so only ADA ratings are presented. Column 1 of Table 2 summarizes the medians by listing the number of points of deviation of the committee median from the House median. A positive score designates a relatively liberal committee, a negative score a relatively conservative one. Committees that are somewhat more *conservative* than the House include Agriculture, Veterans' Affairs, Merchant Marine, Small Business, Science and Technology, and Standards of Official Conduct. The Armed Services Committee, however, is a more significant conservative outlier, with a median 27.5 points lower than the House's.

Several additional findings emerge from the remaining columns of Table 2. Most standing committees do not have general ideology medians or means significantly different from the House. Column 2 shows that for 15 of the 22 standing committees fewer than 10% of the House's members lie in the gap between the House and committee medians. The paucity of general ideology outliers is also reflected in column 3, which summarizes the hypothesis tests for differences in means. Generously overlooking differences in signs and thus allowing maximum opportunity to reject the null hypothesis, the t-statistics are significant at the .05 level for only five committees: Foreign Affairs, Education and Labor, Post Office and Civil Service, Armed Services, and District of Columbia. Of these, only Armed Services is currently among the five most desirable assignments as estimated by Munger (1988, tbl. 3), and here the putative self-selection runs contrary to the majority party's preferences. In contrast to the handful of outliers, committees that appear to be microcosms of the House include Ways and Means, Budget, Appropriations, Agriculture, Public Works, and Energy and Commerce. Indeed, Davidson (1981) singles out the first three of these committees as exceptions to his generalization about outliers.

While committees exhibit substantial variation in terms of whether their means and medians are outliers and, if so, whether they are in a conservative or liberal direction, their composition is heterogeneous almost without exception. This claim is supported by the fact that the committee's party medians lie on opposite sides of the chamber median for every committee except Armed Services. Similarly, the *F*-tests reported in column 4 permit confident rejection of the null hypothesis, H_2°, only in the case of Armed Services, though the Agriculture and Veterans' Affairs Committees are somewhat more homogeneous than the House.[5]

The general ideology results raise some preliminary doubts about prevailing views on preference outliers. Cross-committee variation in outlying tendencies is evident, but the classical homogeneous high-demand committee seems at first glance to be an endangered species. These results must be interpreted with extreme caution, however. The most apparent problem is that the exceptional diversity of votes on which ADA ratings are based may mask some significant jurisdiction-specific differences in committee-House preferences. This possibility is examined next.

Policy-specific Results

Table 3 presents results for nine standing committees and one select committee for which jurisdiction-specific ratings are available. Again, the best support for the classical outlier view is the Armed Services Committee. American Security Council (ASC) scores, which focus exclusively on issues of foreign policy and defense, depict a committee whose members tend to be substantially more conservative than a majority of the House. Indeed, over one-fifth of the House membership has ratings between the two medians. Republicans and Democrats alike are outliers in the committee's jurisdiction, and they form a statistically significant homogeneous group, as reflected

Table 3. Preference Outliers Based on Policy-specific Ratings

Committees	Rating	Difference in Medians	Percentage in Gap	*t*-statistic for H_1^o	*p*-value for H_2^o
Armed Services	ASC	50.0	21.4	4.93	.02
Appropriations	BIPAC	-13.5	8.3	-1.95	.26
	CCUS	-15.0	13.3	-2.68	.12
Budget	BIPAC	-3.0	1.4	-.21	.70
	CCUS	-2.0	.7	-.51	.65
Education and Labor	COPE	19.0	11.5	1.14	.61
	IBT	15.0	3.2	1.22	.37
	MNPL	10.5	6.7	.97	.61
	RLEA	8.0	1.8	.45	.48
	NEA	2.5	.5	.95	.24
Foreign Affairs	ASC	-30.0	8.0	-1.62	.54
Interior	CCUS	-10.5	7.1	-.11	.72
	LCV	-3.0	.0	-.13	.98
	PCCW	-5.0	2.1	-.38	.82
Public Works	CCUS	-4.5	2.5	-.57	.55
	LCV	-3.0	.0	-1.12	.16
	PCCW	-4.0	.9	-.13	.29
	RLEA	.0	.0	.55	.47
Small Business	BIPAC	-8.0	3.0	-.42	.47
Agriculture	NFU	6.0	4.6	1.62	.01
Aging (Select)	NCSC	3.5	.9	1.36	.19

Note: All party medians are heterogeneous except for Armed Services, for which the medians are homogeneous.

by the *p*-value of .02. These findings differ from at least one prior study of Armed Services, which characterized that committee as a relatively *low* demander, who battled, often successfully, with relatively high-demand Appropriations subcommittees (Stephens 1971). But it is consistent with more recent studies that claim support for high demanders on Armed Services (Ray 1980; Weingast and Marshall 1987). Two caveats should be issued, however. First, the ASC ratings are based on only 10 votes. Second, the votes for the Ninety-ninth Congress sharply divided House members. The *F*-test presumes normality of the parent distribution; and, for ASC ratings, this condition appears from the sample not to be met. Moreover, impressionistic evidence is suggestive of heterogeneity on Armed Services, contrary to the *F*-test. One member is Representative Ronald Dellums, a Democrat from Berkeley, whose 1986 ASC and ACU ratings are 0 and ADA rating is a perfect 100. Three other members also had ASC ratings of 0: Nicholas Mavroules (D-MA), Dennis Hertel (D-MI) and Thomas Foglietta (D-PA), each of whom is very liberal on ADA and ACU ratings as well.

After Armed Services, evidence of homogeneous high-demand outliers is almost nonexistent. Possible candidates include Appropriations, Education and Labor, and Foreign Affairs. But on closer inspection none of this support is fully convincing.

The Appropriations Committee is a

moderately liberal and statistically significant outlier according to the Chamber of Commerce (CCUS) and Business and Industry Political Action Committee (BIPAC) ratings. These findings are consistent with Schick's (1980) postreform characterization of Appropriations members as "claimants" and diverge from Fenno's (1966) prereform characterization of Appropriations members as "guardians." But the outlying tendency is less pronounced in terms of the percentages of members lying between the chamber and committee medians, and neither rating yields significant differences in variances. To the degree that the Appropriations Committee has been forced to share much of the spending spotlight in the 1980s, the results for the Budget Committee are also relevant here. From all indications (and also consistent with Schick), the Budget Committee is a microcosm of the House. Divergence in chamber-committee preferences is minimal in terms of both means and medians, and the committee is clearly heterogeneous.

The Education and Labor Committee appears to be somewhat of an outlier according to the AFL-CIO's COPE score, with 11.5% of members lying between the committee and House medians. But this difference is not significant according to the t-test. Nor are the committee's liberal leanings strong according to other labor ratings: the International Brotherhood of Teamsters (IBT), the Machinists' Nonpartisan Political League (MNPL), and the Railway Labor Executive's Association (RLEA). On the education issues, the committee and chamber medians are virtually indistinguishable. In light of Fenno's (1973) study and the pattern of results thus far, it is not surprising that the Education and Labor Committee has party medians on the opposite sides of the House median. But variances for Education and Labor tend to be somewhat *less* than those for the House, which may be surprising given the high degree of partisanship in the period Fenno studied. Thus, not only does the Education and Labor Committee fail to live up to the classical outlier claim, it is not a bipolar outlier either.

The Foreign Affairs Committee has persisted in its liberal leanings in foreign policy (Fenno 1973); but here, too, the difference in means falls short of the .05 level of significance. Moreover, when compared with Armed Services, this result raises questions that conventional preference outlier claims do not answer. If the Armed Services and Foreign Affairs Committees have similar jurisdictions in a manner that the ASC ratings tap, why are their members outliers on *opposite* sides of the spectrum—consistent with the majority party's preferences in one case but contrary to them in the other? And why is one committee, but not the other, significantly more homogeneous than the House? In short, it is increasingly apparent that the committee assignment mechanism operates neither automatically nor uniformly.

The remaining committees in Table 3 deal most explicitly in constituency-specific benefits and thus provide what should be the best opportunity to marshal support for the classical view. As such, the absence of strong and uniform support is striking. Regardless of the policy-specific rating employed, the quintessential pork barrel committees—Interior and Public Works—are not homogeneous high demanders at all.[6] Nor is there evidence of self-selection of high demanders on the Small Business Committee. On Agriculture, ratings of the National Farmers' Union provide the most jurisdiction-specific information of all ratings; yet even here the results are inconclusive. The *t*-statistic approaches significance at the .05 level, but only 4.6% of House members have ratings between the committee and chamber medians. And although they *F*-test uncovers significant homogeneity, the committee is nevertheless heterogeneous according to the party median criterion. At best, then, the Agriculture Committee is a somewhat homogeneous medium-high demander.

Finally, if self-selection is apparent anywhere, it should be on a committee designed to represent voters who turn out at high rates and on which, as a select committee without legislative powers, work load is low and position-taking opportunities are high. Yet the Select Committee on Aging is only slightly more predisposed to policy favorable to senior citizens than is the House overall.

Additional Evidence

Are the findings reported above for the House in the Ninety-ninth Congress atypical? Do Senate committees conform to preference outlier claims better than House committees do? Comparable data from the Ninety-sixth through Ninety-eighth Congresses (and Ninety-ninth for the Senate) clearly say *no.*

According to general ideology measures, the Ninety-ninth Congress is much like the Ninety-sixth to Ninety-eighth Congresses. Armed Services is consistently more conservative than the House. Veterans' Affairs and Public Works join the conservative list for late 1970s Congresses but become moderate thereafter. In contrast, the Education and Labor, Foreign Affairs, and District of Columbia committees are sometimes significantly more liberal than the House.

Jurisdiction-specific special interest ratings exhibit only a few minor deviations from the pattern discovered in the Ninety-ninth Congress. While Armed Services was always high on the ASC scale, it was clearly *not* homogeneous in the Ninety-sixth and Ninety-seventh Congresses. Agriculture was a barely significant outlier in the Ninety-sixth Congress according to the NFU ratings, but this finding has since dissipated. Similarly, Education and Labor exhibited some outlying tendencies on labor ratings in the Ninety-sixth Congress but was less prolabor during the Ninety-seventh and not significantly different from the House in the Ninety-eighth and Ninety-ninth Congresses. Moreover, it was clearly heterogeneous throughout the period. Public Works was significantly antienvironment according to the LCV ratings in the Ninety-sixth Congress; but in each succeeding Congress, the results from the Ninety-ninth are typical of this *non*outlying pork barrel committee. Finally, the Select Committee on Aging was (barely) significantly predisposed to senior citizens in the Ninety-seventh and Ninety-eighth Congresses; but here, too, heterogeneity of preferences was characteristic of the committee in each Congress. Thus, for the eight-year period beginning in 1979, there are almost no instances of homogeneity of committee preferences in the House and only occasional instances of significant outliers.

For the Senate, the data reveal more of the same. In the Ninety-ninth Congress, for example, not a single standing committee in the Senate was a general ideology outlier. Special interest ratings also reveal a robust pattern of moderation in Senate committee preferences. Even the somewhat conservative Senate Armed Services Committee fell short of the .05 significance level on the ASC scale. Instead, the closest thing to an outlier in the Senate seems to be the Environment and Public Works Committee, which is somewhat more proenvironment than the Senate according to LCV ratings (see Table 3). Finally, the Senate Agriculture Committee is somewhat (but not significantly) more *anti*agriculture than the Senate according to NFU scores.

Discussion

The composite picture of committees that has emerged from congressional literature has, in some circles, attained the status of a "stylized fact": self-selection in the committee assignment process results in committees that are composed of homogeneous preference outliers who secure disproportionate policy benefits. At minimum, the results of this study force us to entertain the possibility that the standard preference outlier story is a stylized fiction. Few contemporary committees are composed predominantly of high-demand preference outliers; and homogeneity of preferences within committees is the exception, not the rule.

Compared with much of the literature on the composition of congressional committees, these findings are surprising in and of themselves. They also have two more far-reaching implications. First, because it now appears that a necessary condition for committee power is rarely met, empiricists are encouraged to reconsider and, if necessary, rewrite the conventional wisdom on the composition of congressional committees and the nature of committee power. Second, theorists are encouraged to exercise additional caution when employing the now-conventional formal depiction of committees as unitary (homogeneous) actors with extreme (outlying) ideal points.

These findings and interpretations are not immune from objections, of course. To put the study in perspective and to guide future research, I conclude by examining three common criticisms: (1) that the data are suspect; (2) that, while perhaps accurate, the findings are unique features of the contemporary Congress; and (3) that in any case, old theories should not be abandoned until new and better ones come along.

Dubious Data?

Arguments that interest group ratings are problematical span a broad spectrum. Most of the strongest criticisms pertain to the fact that such ratings tend to be based on intermediate stages of the legislative process and therefore are subject to selection bias.[7] On this empirical foundation, various theoretical arguments are constructed. For example, a perfect information model may have the feature that in equilibrium, contentious issues are not subjected to roll call votes on the floor because of committees' gate-keeping powers or restrictive procedures (Denzau and Mackay 1983; Shepsle and Weingast 1987). So, the argument goes, only in committee can one hope to observe behavior that correctly discriminates between legislators in terms of their preferences.

While equilibrium arguments may seem like compelling reasons to doubt the usefulness of interest group ratings, two points are noteworthy. First, a strict interpretation of at least one model that lends itself to such an argument—namely, Shepsle and Weingast's (1987) "ex post veto" model—has the property that "floor events do not constrain the committee in conference and hence are, in effect, irrelevant" (Weingast 1989, 799). Clearly, the theoretically derived irrelevance of amendment activity is difficult to reconcile with the empirically documented quantity and contentiousness of amendment activity (Smith 1989). Second, even if this discrepancy is ignored, the equilibrium argument is faced with another problem that comes closer to the focus and findings of the present study. If, as is claimed, the uniquely meaningful legislative disputes occur in committees, is this not a tacit admission that committee members' preferences are *not* homogeneous? That is, a logical implication of the equilibrium argument is consistent with one of the main empirical findings of this study, committee heterogeneity. Ironically, however, it is inconsistent with a key assumption of the model on which the argument is based, committee *homogeneity*. In summary, it seems unreasonable to discard potentially useful data because of arguments resting on theories that, when subjected to data, seem not to fare well.

Logical exercises aside, the main issue is not whether interest group ratings are without limitations. It is whether the use of such data is reasonable, given the specific purposes of this study. Conceding selection bias and setting aside equilibrium arguments, I would argue that if interest group ratings are based on votes with jurisdiction-specific content, if interest groups regard such votes as important, and if congressmen regard the votes as meaningful, the ratings are, at minimum, not meaningless. More constructively, they provide a unique and reasonable opportunity to reject null hypotheses in favor of alternative hypotheses that are widely believed to be true.

Contemporary Artifacts?

This study has focused explicitly and exclusively on the Ninety-sixth to Ninety-ninth Congresses. Although it seems doubtful whether committees' composition was somehow atypical during this eight-year period, this is a natural topic for future research. Those who pursue it may continue to entertain beliefs that classical preference outliers once thrived and theorize about historical change.

What might this research look like? Suppose that similar data were collected for the 1950s and 1960s and that convincing evidence of preference outliers was found. What might account for the change in the 1970s? Formal theorists are fond of explanations that are potentially amenable to refutation by testing comparative statics predictions derived from the equilibrium of a given model. Scientific and sound as this can be, a word of caution is in order. Properly conducted empirical research of this sort requires that exogenous variables of the model be explicitly identified and be shown to have changed in value during the historical period. Otherwise, these essential theory-testing efforts run the risk of causing more confusion than clarity.[8]

Abandon Old Theories?

The paucity of homogeneous high-demand committees in the Ninety-sixth through Ninety-ninth Congresses is an empirical finding, first and foremost. Its theoretical implications, while significant, fall well short of abandoning all existing models in which a legislative committee is represented as a unitary actor. Rather, the practical and constructive modeling concerns are whether more realistic assumptions are tractable and—if and when they are not—whether the unitary actor assumption yields predictions that survive empirical tests.

Recent game-theoretic research illustrates that formal characterizations of different types of committees are tractable (Gilligan and Krehbiel 1987, 1989). The latter article analyzes an incomplete information model in which a heterogeneous committee consists of two actors whose ideal points are on opposite sides of the ideal point of the legislature's median voter. Compared with the earlier article's homogeneous committee (unitary actor), heterogeneous committees are preferred by the legislature because of their greater willingness to share their policy expertise. Furthermore, under either assumption about committee types (homogeneous or heterogeneous), the legislature has an incentive *not* to allow its committees to be composed of preference outliers (homogeneous outliers in the first case, bipolar outliers in the second). In light of the empirical results reported above, this form of theoretical analysis seems promising.

Although new, tractable, and perhaps more realistic assumptions exist, the unitary actor assumption does not need to be abandoned. Nor do those who continue to employ the assumption have to adhere to rigid, literal interpretations of it.

Again, the issue is ultimately empirical. Notwithstanding the heterogeneity of actual committee members, do they nevertheless somehow behave *as if* they make up a homogeneous entity? Excellent examples in which they might are provided in Fenno's (1966) account of the House Appropriations Committee (during some years) and Manley's (1970) account of the Ways and Means Committee under the chairmanship of Wilbur Mills. One caveat remains, however. If a committee fundamentally is not an outlier, it seems implausible that it would behave as if it were an outlier.[9] Thus, the as-if defense, while effective in some contexts, ought not to be relied upon perfunctorily.

Conclusion

Because most of the findings of this study are consistent with *null* hypotheses, the final word on preference outliers almost surely has yet to be uttered. Indeed, new and diverse directions for legislative research on committees and their composition have emerged. Preference outlier stalwarts may continue to believe that congressional committees are composed of homogeneous high-demanders. However, a greater burden of proof now accompanies such beliefs. Converted or calcified preference outlier skeptics, on the other hand, have a distinctly different mission—to rethink the role of standing committees in the legislative process, to entertain theoretical foundations that are not biased by a presumption of committee power, and to develop alternative theories in which homogeneous high-demand committees arise endogenously if they arise at all.

Notes

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1. A third variation on this means variance theme is a *uniform shift outlier*—a committee whose members, while no more homogeneous than the parent chamber, are systematically higher demanders. In this case we should observe a difference in means or medians but not in variances. While theoretically possible, this pattern has received little attention and thus will not be considered.

2. Data were obtained from Legi-Slate, a subsidiary of the *Washington Post*. These ratings differ slightly from some of those obtained directly from interest groups because, unlike some interest groups, Legi-Slate does not count absences as "incorrect" votes during computation.

3. The test is based on the assumption that interest group ratings are independent draws from a hypothetical distribution of possible ratings. Thus, failure to reject the null hypothesis indicates that the observed differences could have occurred by chance when taking independent draws from the distribution.

4. To detect bipolar outliers, tests were also conducted with the same null hypothesis but with the alternative hypothesis $\sigma_c^2 > \sigma_h^2$. Here the test statistic is $s_c^2/s_h^2 \sim F_{N_c,N_h}$. These results will be reported in passing whenever statistically significant.

5. Since critical values of the *F*-statistic differ both across committees (because of different degrees of freedom) and across ratings (because of different parent samples), I report the *p*-value for each committee. This is the probability of rejecting the null hypothesis when it is true. Support for the homogeneity hypothesis at the .05 level therefore requires a $p \leq .05$. For each committee *F*-tests were also conducted for the bipolar outlier hypothesis (see n. 4), but statistically significant support was nonexistent.

6. Only in the case of Interior and LCV scores was a committee significantly *more* heterogeneous than the parent body, thus supporting the bipolarity outlier hypothesis. The difference between s_c^2 and s_h^2 was significant at the .02 level.

7. See VanDoren 1988 for a discussion of this point and empirical analysis in the context of the literature that seeks to differentiate between self-interested and ideological voting.

8. An example can be found in the controversy Krehbiel, Shepsle, and Weingast 1987, in which Shepsle and Weingast defended their ex post veto model as having "as its empirical referent the House committee system of the 1950s and 1960s" (p. 941) and advocate comparative statics as a way of testing it. A *necessary* condition for such analysis to establish that the ex post veto was the "institutional foun-

dation of committee power" in the fifties and sixties is that the exogenous variable for the comparative statics—namely, the existence or nonexistence of an ex post veto—indeed varied during the time series. As a point of fact, changes in conference procedures during the reform years were minimal and had no bearing on the existence or nonexistence of an ex post veto.

9. In contrast, the opposite is quite plausible. A genuinely outlying committee, because of expectations about floor behavior, may propose bills coincident with the floor median voter's ideal point, as if the committee were moderate. This seems to characterize the Senate's consideration of minimum wage legislation in 1977 (Krehbiel and Rivers 1988).

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SEQUENTIAL VOTING WITH ENDOGENOUS VOTER FORECASTS

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We investigate a sequential voting model in which voters' forecasts of outcomes on future issues are determined endogenously. Voters are assumed to make decisions in an environment in which future outcomes are uncertain. The uncertainty arises from two possible sources. Voters may be uncertain of other voters' preferences. In addition, events that occur before future issues are decided may affect voters' preferences on future votes. Information available to voters at each point in time is characterized. An equilibrium to the voting problem, if one exists, is one for which the outcome on the issue currently being decided and voters' forecasts of outcomes on future issues are determined simultaneously. We show that an equilibrium exists under a particular voting institution and characterize voters' forecasts for this equilibrium.

Several recent studies have investigated the properties of majority rule voting processes when issues are voted on sequentially. The motivation for this approach is twofold: (1) to investigate the role of institutional structure in influencing outcomes and (2) to determine whether problems of nonexistence of equilibrium in multidimensional voting problems may be overcome by assuming a particular structure for voting decisions (see Denzau and Mackay 1981; Feld and Grofman 1987; Kramer 1977; Plott 1967).

Denzau and Mackay (1981) show that existence of equilibrium and the properties of equilibrium when it exists depend not only on voters' preferences but also on their expectations about outcomes of future votes. In particular, they show that an equilibrium may fail to exist if voters have perfect foresight expectations. Moreover, when preferences are such that an equilibrium exists with perfect foresight expectations, the equilibrium will generally differ from the one that would prevail under myopic expectations. Their analysis and subsequent analyses (including the present one) assume voters have weighted Euclidean distance (Enelow and Hinich 1984) preference functions.

Enelow and Hinich (1983) study an environment in which voters forecast outcomes on future votes with error. They show that voter preferences on each issue are single-peaked if each voter's forecast of outcomes on future votes does not depend on the outcome currently under consideration. Enelow (1984) shows that this result can be generalized. Preferences are single-peaked if voters' forecasts of outcomes on future votes depend linearly on the issue currently under consideration, and the variance of their forecasts does not depend on the outcome of the issue currently being decided.

Enelow and Hinich (1983) and Enelow (1984) take voters' forecasting rules as given exogenously, that is, outside the model. However, in light of their results, it is natural to ask whether the sequential voting model can be augmented by informational assumptions that permit voters' forecasting rules to be derived endogenously, that is, within the model. That is the problem studied here. We assume that voters are in an environment in which future outcomes are uncertain. The uncertainty arises from two possible sources. Voters may be uncertain of other voters' preferences. In addition, events that occur before future issues are decided may affect voters' preferences on future votes. We characterize information available to voters at each point in time. If there is an equilibrium to the voting problem, it is one in which, for each issue, the outcome on that issue and voters' forecasts of outcomes on future issues are determined simultaneously. We show that an equilibrium exists under a particular voting institution, and we characterize voters' forecasts for this equilibrium.

We know from Denzau and Mackay (1981) that there are conditions under which equilibrium will fail to exist when voters have full information and voters' forecasts are endogenous. Hence, it is natural to expect that there will be conditions under which existence of equilibrium will fail when voters make endogenous forecasts with imperfect information. In addition to studying a class of problems for which equilibrium exists, we indicate the problems that arise in attempting to extend our analysis beyond that class.

A model with endogenous voter forecasts is important for the following reasons. Over time, voters are continually receiving new information about the preferences of constituents or other voters (opinion polls), the state of the domestic economy, international events, and a variety of other factors that may affect their preferences about issues yet to be decided. A model with endogenous voter forecasts can provide insights into how voters use information and how voters' forecasts and the associated voting outcomes change as new information becomes available. Hence, a model with endogenous voter forecasts can provide valuable insights into the role of information in political decisions.

In a model with sequential voting, voter forecasts play a central role in determining the sequence of outcomes. If voter forecasting rules are taken to be exogenous and permitted to change in arbitrary ways between votes, the potential for testing the model is quite limited. Any sequence of observed outcomes could be explained by a suitable choice of parameters in voters' forecasting rules. Hence, formulating a model of how voters' forecasts are determined is an important part of the overall goal of modeling outcomes when issues are decided sequentially. Hence, we believe that making voter forecasts endogenous is the next logical step in building on the insights of Denzau and MacKay (1981), Enelow and Hinich (1983), and Enelow (1984).

Having argued that forecasting rules should be made endogenous, we still have a good deal of latitude in modeling the way in which forecasts are formed. For example, we might simply assume that voters choose forecasting rules before the first issue is decided and adhere to those rules regardless of new information obtained as time passes. Alternatively, we might assume that voters adjust adaptively, changing the coefficients in their forecasting rules in some prescribed way when the outcome they observe differs from their ex ante forecast. These and similar approaches have the shortcoming that voters can make systematic errors without revising their approach to the forecasting problem.

Our approach is based on the assumption that voters use their knowledge of the way in which issues are decided to forecast future outcomes. Implementing this approach requires us to be explicit about what information voters have at each point in time about their own preferences, the preferences of other voters, and other factors relevant to the voting decision. Given this information and their knowledge of the way in which issues are decided, voters can then forecast future outcomes. Their forecasts thus make use of all information available to them.

The Model

A series of $T \geq 1$ issues are to be decided sequentially by N voters. The outcome on each issue is determined by a voting procedure detailed below. Once an issue is decided, it cannot be reconsidered. Decisions are implemented and consumption occurs after all issues have been decided. The order of voting on the issues is fixed.

Individual voters are endowed with private information, ϕ_i, about their own preferences. This information is drawn from a distribution $f(\phi)$. In developing our analysis, we assume that the distribution $f(\bullet)$ is known to all voters. In our remarks, we discuss the implications of relaxing this assumption. After the outcome on each issue has been determined, new information may become available. Let ω_t be the new information received after issue t is decided, and let $\omega = (\omega_1, \omega_2, \ldots, \omega_T)$. It is common knowledge (Aumann 1976) that ω is drawn from the distribution $g(\omega)$, and the draw itself is observed by all voters. Preferences over the T-dimensional vector of outcomes θ are given by

$$u(\theta; \phi_i, \omega) = -[\theta - x(\phi_i, \omega)]'A[\theta - x(\phi_i, \omega)], \quad (1)$$

where the vector $x(\phi_i, \omega)$ is the voter i's ideal point, and A is a positive-definite matrix of salience weights. The functions $x(\bullet)$ are common to all voters.

The following are two examples of special cases conforming to the above structure.

Example 1

A set of N voters is selected at random from a population in which individuals have different ideal points. The distribution of ideal points, ϕ_i, in the population is given by $f(\phi)$. There is no common shock to preferences. In this example, $x(\phi_i, \omega) = \phi_i$.

In this example, the uncertainty about future outcomes arises because voters do not know the preferences of other voters. However, from knowledge of the distribution of preferences in the population, they will be able to form a forecast of outcomes on future votes, as we show below.

In many settings, the outcomes on future votes will be uncertain not only because voters are not entirely sure of other voters' preferences but also because events that may occur before the future votes are decided may affect preferences over future outcomes. A heightened or diminished level of conflict elsewhere in the world may affect willingness to spend for defense. New information about trade deficits or surpluses may affect preferences for protectionist legislation. The emergence of a recession may affect willingness to fund unemployment compensation programs. Disruptions in the supply of oil from abroad may affect willingness to spend resources to develop technologies for alternative energy sources. The next example illustrates how uncertainty about future events might be captured in the structure presented.

Example 2

A set of N voters is drawn at random from a population with direct utility function

$$U(\theta, q_i; z_i, A) = -(\theta - z_i)'A(\theta - z_i) + q_i,$$

where θ is publicly provided goods, q_i is consumption of private goods, and z_i and A are preference parameters. The publicly provided goods θ will be financed by a proportional income tax. Thus,

$$q_i = y_i(1 - \tau), \tag{2}$$

where y_i is i's before-tax income and τ is the tax rate. Suppose there are two publicly provided goods and let θ_1 and θ_2 be funds spent on these two activities. The public sector budget constraint is then

$$\tau Y = \theta_1 + \theta_2, \tag{3}$$

where Y is aggregate income. Suppose i's relative position in the income distribution is given by

$$\frac{y_i}{Y} = k_i + \delta\epsilon_i, \tag{4}$$

where δ is a common shock to income (associated with, say, fluctuations in the aggregate economy), ϵ_i is an individual-specific variable reflecting the sensitivity of individual i's earnings to business fluctuations, and k_i is an individual-specific variable reflecting the individual's position in the income distribution in the absence of shock δ. Here $\Sigma k_i = 1$ and $\Sigma\epsilon_i = 0$.

Solve equation 3 for τ, substitute the result into equation 2, and replace y_i/Y by the right-hand side of equation 4 to yield an indirect utility function of the form in equation 1 (up to an individual-specific additive constant) with ideal points:

$$x_i = z_i - (k_i + \delta\epsilon_i)\,\frac{A^{-1}}{2}\,e', \tag{5}$$

where $e = (1, 1)$. In this example, $\phi_i = (k_i, \epsilon_i, z_i)$, $\omega = (\delta, Y)$, and $x(\phi_i, \omega)$ is given by equation 5. The example illustrates how fluctuations in the state of the economy δ and Y affect voters' ideal points over issues.

Analysis of voting with incomplete information poses difficulties not encountered in models with full information (Ordeshook and Palfrey 1988). In order to characterize equilibrium in the sequential voting problem, we assume the following voting procedure is employed. When an issue is considered, each voter submits a proposal $v_i \in R$. The outcome on the issue is

$$\operatorname*{med}_i\{v_i\}.$$

We call the proposal that i submits i's vote. On each issue each voter observes the outcome. Our results do not depend on whether voters observe the proposals made by other voters. Hence, the results apply to either secret ballot elections or to elections where all votes are revealed.

This voting procedure clearly does not capture the richness of information transmission and agenda formation in committee decision making (Austen-Smith 1988). However, this approach permits us to sidestep some of the complexity of models of agenda formation. The procedure proves to be a useful vehicle for gaining insights into models such as the sequential-voting model we study.

In this model, a player i's strategy maps the history of the game (the outcomes on past votes) and player i's type (the vector ϕ_i) into a vote on the current issue. We consider the Nash equilibrium that emerges by solving the model using backward induction. It is a property of our model that a voter's optimal decision at each stage of the game is the same regardless of the voter's beliefs about other voters' types (i.e., the ϕ_i's of other voters) at that stage of the game. Hence, we do not need to make any particular assumption about how a voter updates beliefs about other voters' types as the game proceeds. It is natural to assume, however, that beliefs are updated using Bayes's rule, in which case, our solution to the game is a sequential equilibrium (Kreps and Wilson 1982).

We next present some definitions and

results that prove useful in the analysis to follow.

DEFINITION. *For utility function $U_{1i}(\cdot)$ for voter i, v_i^* is a dominant strategy if*

$$U_{1i}(med\{v_i^*, v_{-i}\}) \geq U_{1i}(med\{v_i, v_{-i}\}) \quad \text{for all } v_i, v_{-i}. \qquad (6)$$

Here v_i is i's vote and v_{-i} is the vector of votes by all other voters.

THEOREM 1 (Black 1958). *If $U_{1i}(\cdot)$ is single-peaked, i has a dominant strategy, $v_i^* = \underset{x}{Argmax}\, U_{1i}(x)$.*

COROLLARY. *If $v_j = v_j(\epsilon)$ for $j \neq i$, where ϵ is a random vector and $U_{1i}(\cdot)$ is single-peaked, $v_i^* = \underset{x}{Argmax}\, U_{1i}(x)$ is a dominant strategy for i regardless of the process that generates ϵ.*

Proof. Since v_i^* satisfies equation 6 for all v_{-i}, it follows that $E\, U_{1i}(med\{v_i^*, v_{-i}(\epsilon)\}) \geq E\, U_{1i}(med\{v_i, v_{-i}(\epsilon)\})$. QED

LEMMA 1. *If y is a random variable that emerges when all players follow their equilibrium strategies in the future, i expects all players to pursue their equilibrium strategies in the future, and i's utility over present and future outcomes can be written*

$$U_i(med\{v_1, v_2(\epsilon), \ldots, v_n(\epsilon)\}, y) = U_{1i}(\underset{i}{med}\{v_1, v_2(\epsilon), \ldots, v_n(\epsilon)\}) + U_{2i}(y)$$

where $U_{1i}(\cdot)$ is single-peaked, then $v_i^* = \underset{x}{Argmax}\, U_{1i}(x)$ is i's best proposal for the current issue.

Proof. Obvious extension of the corollary. QED

To illustrate the essential features of our analysis, we begin by solving Example 1 for the case in which only two issues are to be decided. For notational convenience, let $x_i = x(\phi_i) = \phi_i$.

At the time that issue 2 is decided, the outcome on issue 1, θ_1 is known. With θ_1 given, the utility function in equation 1 is concave in θ_2 and, hence, single-peaked. By the corollary to Theorem 1, i's dominant strategy is to vote the value that maximizes equation 1 with respect to θ_2. Voters i's most preferred outcome on issue 2, given outcome θ_1 on issue 1, is found by setting the derivative of equation 1 with respect to θ_2 equal to zero and solving to obtain

$$v_{i2}^* = \alpha\theta_1 + \beta_i, \qquad (7)$$

where

$$\alpha = -(a_{12}/a_{22}) \qquad (8)$$

and

$$\beta_i = -\alpha x_{1i} + x_{2i}. \qquad (9)$$

The median outcome of the period 2 vote is then

$$\tilde{\theta}_2 = \underset{i}{med}(\alpha\theta_1 + \beta_i). \qquad (10)$$

Since α and θ_1 do not depend on i, this may be rewritten as

$$\tilde{\theta}_2 = \alpha\theta_1 + \underset{i}{med}\, \beta_i \qquad (11)$$

Now $\tilde{\theta}_2$ is a random variable, the distribution of which is derived from the distribution of the x_i. The voter's first-period optimization problem is obtained by inserting $\tilde{\theta}_2$ in place of θ_2 in equation 1 and taking expectations. Voters have differing information (each voter knows his or her own preferences but not the preferences of others). Hence, the perceived distribution of future outcomes conditional on the information each voter has may differ across voters. In light of the results of Enelow and Hinich (1983) and Enelow (1984), it is interesting to observe that the mean of $\tilde{\theta}_2$ is linear in θ_1.

It is also of interest to note how the structure of the voting problem affects the forecasting equation 11. In particular, from equation 8, the coefficient of θ_1 in equation 11 is determined by elements of the salience matrix A. Also, the random component in equation 11 arises from voters' uncertainty about other voters' preferences, as is evident from equation 9. Hence, the distribution of voter forecasts in this example is determined by the distribution of voter preferences through equation 11.

Voter i's most-preferred outcome on issue 1 is obtained by choosing v_i to maximize:

$$-E\{[a_{11}-(a_{12}^2/a_{22})](\theta_1 - x_{1i})^2\}$$
$$- Ea_{22}[\beta - x_{2i} - (a_{12}/a_{22})x_{1i}]^2, \quad (12)$$

where the expectation in equation 12 is taken conditional on information available to voter i and

$$\beta = \operatorname*{med}_j \beta_j.$$

In particular, voter i knows $f(\bullet)$ and $g(\bullet)$ as well as his or her own preferences. The second expression in equation 12 does not depend on i's vote on issue 1. The first expression in equation 12 is single-peaked in θ_1. Hence, Lemma 1 applies. Voter i's most-preferred outcome on issue 1 is $v_{1i}^* = x_{1i}$, and the outcome on issue 1 is

$$\tilde{\theta}_1 = \operatorname*{med}_i\{x_{1i}\}. \quad (13)$$

In this example, the outcome on the first issue to be voted on (issue 1) is simply the median of the most-preferred values on that issue (equation 13). By contrast, the outcome on the second issue to be voted on (issue 2) depends on the outcome on the first issue (equation 11). Thus, the order of voting will often matter. It follows that the sequential voting outcomes need not be the same as outcomes obtained if the issue space were rotated so that voters' salience weights in the rotated space were diagonal. In the rotated space voting outcomes would be invariant to order while in the original space outcomes will often depend on order.

We now generalize to the case of an arbitrary number of issues. We will make use of the following linear algebra result.

LEMMA 2. *Consider the quadratic form*

$$Q(y, z) = [(y - \mu_y)', (z - \mu_z)'] \begin{bmatrix} \Sigma_{11} & \Sigma_{12} \\ \Sigma_{21} & \Sigma_{22} \end{bmatrix} \begin{bmatrix} y - \mu_y \\ z - \mu_z \end{bmatrix}$$

where Σ_{11} is nonsingular and $\Sigma'_{12} = \Sigma_{21}$. This quadratic form can be rewritten

$$Q(y, z) = (y - d)'\Sigma_{11}(y - d) + (z - u_z)'\Sigma^{22}(z - \mu_z),$$

where

$$d = \mu_y - \Sigma_{11}^{-1}\Sigma_{12}(z - \mu_z)$$

and

$$\Sigma^{22} = \Sigma_{22} - \Sigma_{21}\Sigma_{11}^{-1}\Sigma_{12}$$

Proof. Substitute d and Σ^{22} in the second expression. Straightforward algebra yields the first expression. QED

Lemma 2 has a statistical interpretation. The first expression for $Q(y,z)$ appears in the exponent of a jointly normal distribution for (y,z), with mean (μ_y, μ_z) and covariance matrix

$$\Sigma = \begin{bmatrix} \Sigma_{11} & \Sigma_{12} \\ \Sigma_{21} & \Sigma_{22} \end{bmatrix}$$

The second expression for $Q(y, z)$ appears in the exponent of the marginal distribution for z and the conditional distribution of y, given z (see, e.g., DeGroot 1970, 54–56).

For simplicity, we present our proof for the case in which there is no common shock, ω. When the proof is complete, we indicate how the result can be generalized to include the common shock. We next present several definitions used in proving our major result.

Define matrices B^s, $s = 1, 2, \ldots, S$ recursively as follows:

$$B^S = A$$

$$B^{s-1} = B^s_{22} - B^s_{21}(B^s_{11})^{-1}B^s_{12}, \quad s = 2, \ldots, S$$

where

$$B^s = \begin{bmatrix} B^s_{11} & B^s_{12} \\ B^s_{21} & B^s_{22} \end{bmatrix}, s = 1, 2, \ldots, S.$$

B^s_{11} has dimension 1 × 1, B^s_{21} has dimension $(s - 1) \times 1$, B^s_{12} has dimension $1 \times (s - 1)$, and B^s_{22} has dimension $(s - 1) \times (s - 1)$. Note that $B^1 = B^1_{11}$. Hence, define $B^1_{21} = B^1_{12} = B^1_{22} = 0$.

Let

$$\Theta_s = [\theta_s, \theta_{s-1}, \ldots, \theta_1, \theta_0], \quad s = 0, 1, 2, \ldots, S, \text{ where } \theta_0 \equiv 0.$$

Let

$$X_{is} = [x_{is}, x_{is-1}, \ldots, x_{i1}, x_{i0}], \quad \forall i, s = 0, 1, 2, \ldots, S,$$

where $x_{i0} \equiv 0 \; \forall i$. Let

$$m_{is} = x_{is} + (B^s_{11})^{-1}B^s_{12}x_{is-1} \quad \forall i, s = 1, 2, \ldots, S \tag{14}$$

$$m_s = \operatorname*{med}_i\{m_{is}\}, s = 1, 2, \ldots, S \tag{15}$$

$$M_{is} = \begin{cases} \sum_{t=s}^{S-1} (m_t - m_{it})' B^t_{11}(m_t - m_{it}), & \forall i, s = 1, 2, \ldots, S - 1 \\ 0 & s = S \end{cases} \tag{16}$$

Define the functions

$$v_{is}(\Theta_{s-1}) = x_{is} - (B^s_{11})^{-1}B^s_{12}(\Theta_{s-1} - X_{is-1}), \quad \forall i, s = 1, 2, \ldots, S$$

$$v_s(\Theta_{s-1}) = \operatorname*{med}_i v_{is}(\Theta_{s-1}), \quad s = 1, 2, \ldots, S$$

Note that $v_s(\Theta_{s-1})$ can be written

$$v_s(\Theta_{s-1}) = -(B^s_{11})^{-1}B^s_{12}\Theta_{s-1} + \operatorname*{med}_i\{x_{is} + (B^s_{11})^{-1}B^s_{12}X_{is-1}\}.$$

Thus, using equations 14 and 15, $v_{is}(\Theta_{s-1})$ and $v_s(\Theta_{s-1})$ can be written

$$v_{is}(\Theta_{s-1}) = -(B^s_{11})^{-1}B^s_{12}\Theta_{s-1} + m_{is}, \forall i, s = 1, 2, \ldots S \tag{17}$$

$$v_s(\Theta_{s-1}) = -(B^s_{11})^{-1}B^s_{12}\Theta_{s-1} + m_s, s = 1, 2, \ldots, S. \tag{18}$$

Let $\tilde{\theta}_0 \equiv 0$. Solve equations 17–18 forward sequentially where, at each $s > 0$,

$$\tilde{\theta}_{is} = v_{is}(\tilde{\Theta}_{s-1}), \; \tilde{\theta}_s = v_s(\tilde{\Theta}_{s-1}),$$

and, for $s \geq 0$,

$$\tilde{\Theta}_s = [\tilde{\theta}_s, \tilde{\theta}_{s-1}, \ldots, \tilde{\theta}_1, \tilde{\theta}_0],$$

where $\tilde{\theta}_0 \equiv 0$. Then,

$$\tilde{\theta}_{is} = -(B^s_{11})^{-1}B^s_{12}\tilde{\Theta}_{s-1} + m_{is} \quad s = 1, 2, \ldots, S \tag{19}$$

$$\tilde{\theta}_s = -(B^s_{11})^{-1}B^s_{12}\tilde{\Theta}_{s-1} + m_s. \quad s = 1, 2, \ldots, S. \tag{20}$$

Define

$$V_{is}(\Theta_s) = -(\Theta_s - X_{is})'B^s(\Theta_s - X_{is}) - M_{is}, s = 1, 2, \ldots, S. \tag{21}$$

THEOREM 2. *Let voters have preferences given by equation 1 with* $x(\phi_i, w) = x(\phi_i) = x_i$, *where* x_i *is drawn randomly from a distribution* $f(x)$. *The form of the preference function and the distribution* $f(x)$ *are known to all voters. For each voter* i, *the vector of preference parameters* x_i *is private information. If issues are decided sequentially by the voting procedure we have adopted, an equilibrium exists. The equilibrium sequence of individual votes and the sequence of vote outcomes are given by equations 19 and 20. When voting on issue* s *with arbi-*

trary history Θ_{s-1}, if voter i expects all voters to pursue their equilibrium strategies in the future, i's utility over outcomes on the current issue, θ_s, is given by equation 21 and i's voting strategy is given by equations 17–18.

Proof. The proof is by backward induction. Since $M_{iS} = 0$ and $B^s = A$, i's utility over outcomes on issue S, given history Θ_{S-1}, is given by $V_{iS}(\Theta_S)$. From Lemma 2 and equations 17–18 and 21, this can be rewritten

$$-[\theta_S - v_{iS}(\Theta_{S-1})]'B^S_{11}[\theta_S - v_{iS}(\Theta_{S-1})] - (\Theta_{S-1} - X_{iS-1})'(B^S_{22})^{-1} (\Theta_{S-1} - X_{iS-1}) - M_{iS}. \qquad (22)$$

The first expression in equation 22 is single-peaked in θ_S, and the remaining terms do not depend on θ_S. By the corollary to Theorem 1, voter i has a dominant strategy when voting on issue S, namely, the strategy that maximizes equation 22 with respect to θ_S. The maximum is given by $v_{iS}(\Theta_{S-1})$, defined in equation 17.

Continuing the induction argument, let voter i's utility function at issue $s < S$ for arbitrary history Θ_{s-1} be $V_{is}(\Theta_{s-1})$. Using Lemma 2, rewrite this function as

$$-[\theta_s - v_{is}(\Theta_{s-1})]'B^s_{11}[\theta_s - v_{is}(\Theta_{s-1})] - (\Theta_{s-1} - X_{is-1})'(B^s_{22})^{-1} (\Theta_{s-1} - X_{is-1}) - M_{is}. \qquad (23)$$

The first term in equation 23 is single-peaked in θ_s. The second term is a constant known to i at the time that the vote on issue s occurs. The third term, M_{is}, is defined in equations 14–18. When choosing a vote on issue s, i wishes to choose a vote to maximize the expected value of equation 23 conditional on i's information set, which includes the past history of votes, Θ_{s-1}, i's own ideal point X_{is}, and any other information that may have been revealed to i prior to submission of the vote on issue s. Let I_{is} denote this information set. Now $E(M_{is} | I_{is})$ does not depend on i's vote on issue s. That is, knowledge by i of his or her own vote on issue s reveals no information about M_{is} not already provided by knowledge of I_{is}. Hence, only the first term in equation 23 is a function of i's vote on issue s. Since the first term is single-peaked in θ_s, Lemma 1 applies, and i chooses the vote that maximizes the first term in equation 23 with respect to θ_s. Hence, i's vote is given by equation 17, and the vote outcome on issue s by equation 18.

Substituting the vote outcome $v_s(\Theta_{s-1})$ for θ_s in equation 23 and noting that

$$v_s(\Theta_{s-1}) - v_{is}(\Theta_{s-1}) = m_s - m_{is},$$

we obtain that i's induced utility function over Θ_{s-1} is $V_{is-1}(\Theta_{s-1})$ as given in equation 21. This completes the induction argument.

The claim that equations 19–20 give the equilibrium sequence of votes and vote outcomes then follows by solving equations 17–18 forward sequentially as was done in defining equations 19–20. QED

Remarks

The equilibrium characterized in the theorem is the unique backward induction equilibrium.

The proof can be extended to the case where there is a common shock, ω, in the following way. Replace x_i in the theorem with $x(\phi_i, \omega)$. Interpret the expectation operator at each step of the proof as the expectation conditional on elements of the vector $\omega = (\omega_1, \omega_2, \ldots, \omega_T)$ that have been revealed to date. The steps of the theorem can simply be repeated with these changes.

The discussion indicates why the proof remains valid when a common shock is introduced. However, the introduction of a common shock raises an interesting issue. All voters know that common shocks will occur before the sequence of votes. They know that the realizations of those shocks will affect their preferences over voting choices. One strategy that voters might adopt is to propose policies that are contingent on the realizations of the common shocks. There are many programs that have such contingencies. Social security benefits are indexed for inflation, expenditures for unemployment compensation are contingent on the number of people who are unemployed, disaster relief expenditures are contingent on the occurrence of disasters, and so on. An interesting problem for future research is to investigate the possibility of introducing contingent policies as objects of voting in a sequential voting framework.

In the model we have considered thus far (with or without the common shock to preferences), voters have differing forecasts of future outcomes because they have private information about their own preferences that may not be available to others. If voters have private information about the distribution of other voters' preferences based on private opinion polls, eavesdropping in legislative cloakrooms, and so on, this would provide a further reason for heterogeneity of voter forecasts. However, the results of the theorem would still hold. In the proof, expectations for each voter are taken to be conditional on the information held by that voter. If voters have private information about other voters' forecasts, the coefficients on past outcomes in voters' forecasting rules would still be common across voters, while the distribution of voter forecasts would continue to differ across voters.

In deriving our results, we have assumed that voters have imperfect knowledge of voters' preferences but that they know the distribution $f(\bullet)$, from which ideal points are drawn. Models in which agents' subjective beliefs about relevant probability distributions are assumed to conform to the objective distributions were first introduced by Muth (1961). He proposed calling expectations formed using this assumption "rational expectations." McKelvey and Ordeshook's (1985a, 1985b) investigations of formation of voter expectations also uses the rational expectations approach.

The rational expectations approach implies that an outside observer who wants to study the behavior of voters can use the actual distributions of relevant random variables to predict voters' forecasts. In this way, a link is provided from the theory to actual voter forecasts. This is not the only way that such a link can be made. However, as we argued in the introduction, it is essential that some link be made if the model is to generate implications regarding voter forecasts that may potentially be tested. The rational expectations approach has proven to be a fruitful approach to generating testable implications in other contexts (Lucas and Sargent 1981).

For an equilibrium to exist, however, it is not necessary to invoke the assumption of rational expectations. The proof is valid as long as voters know that the salience matrix (A in our model) in other voters' preference functions is the same as their own. It is not necessary that voters' subjective assessments of the distribution of ideal points be the objective distribution, voters may make systematic errors in predicting outcomes on future votes. However, it is still the case in our model that it is a dominant strategy for each voter to vote his or her ideal point on each issue. Thus, in the case where there is no common shock to preferences, systematic errors in voter forecasts will not affect the sequence of outcomes. If there are common shocks to preferences and voters' subjective assessments of the distribution of the common shock differ from the actual distribution, an equilibrium will exist; but the sequence of outcomes will, in general, be different from the sequence that emerges when the distribution of shocks is common knowledge.

An important assumption that we have made is that the salience matrix, A, in voter preferences (equation 1) does not differ across voters. This assumption is not easily relaxed. To see why, suppose that matrix A is indexed by i. When the A matrix is indexed by i, the simplification obtained by writing equations 17–18 as a linear function of past outcomes with weights common to all voters is no longer available. This is a critical step in the proof. Problems of nonexistence of equilibrium in Denzau and Mackay's (1981) analysis of the perfect forecast case arise only when the salience matrix A differs across voters. Thus, variation of the A matrix across voters continues to pose a problem for sequential voting models with endogenous voter forecasts. Finding a way to relax the assumption that all voters have the same salience weights is an important issue for future research.

Our model has the property that voters have a dominant strategy on each vote, and our analysis relies heavily on this property. It would be interesting to investigate different voting games, in particular when dominant strategies are not present.

Conclusion

Building on the work of Denzau and MacKay (1981), Enelow and Hinich (1983), and Enelow (1984), we have presented a strategy for deriving endogenous voter forecasts when sequential voting occurs under conditions of uncertainty. Our approach assumes that voters use all available information in making their forecasts. In addition, we assume that voters know the distribution from which preference parameters are drawn. Voters then deduce the distribution of outcomes from their knowledge of this distribution and their knowledge of how decisions are made. Hence, if the voting sequence were repeated with a new draw from the distribution of voters for each sequence, the ex post distribution of outcomes would be the distribution voters use in making their forecasts. It is in this sense that voters in our models do not make systematic errors.

It is natural to be somewhat skeptical about voters' ability to gather and process the information required to behave in the fashion assumed in our derivation of endogenous forecasts. Indeed, this issue arises in most rational expectations models. One response to this criticism has been the argument that voters behave "as if" they are following the strategy that we characterize in our analysis, whether they perform detailed computations or not. Attention has recently been devoted to the question of assessing whether individuals using simple forecasting strategies may "learn" the forecasting rules that emerge in the rational expectations formulation (Marcet and Sargent 1989). This is one avenue for addressing the concern that an analysis such as ours presumes too much of voters. An alternative strategy is to attempt to characterize how the information that voters have or use falls short of that assumed in our analysis. As we indicated, an equilibrium will exist in our model in cases where voters' beliefs about the relevant probability distributions do not conform to the actual distributions; but their forecasts will be systematically in error and the sequence of outcomes may differ from that in the case where the actual distribution are known. Thus, departing from the rational expectations assumption does not pose a challenge for proving existence of equilibrium but rather for deriving potentially testable propositions from the theory.

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MISSING THE ARCHIMEDEAN POINT: LIBERALISM'S INSTITUTIONAL PRESUPPOSITIONS

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Though liberalism has been widely criticized for its attempt to frame a detached judgment of society based on an asocial conception of individuals, insufficient attention has been paid to the particular social and political relationships this search for an Archimedean point presupposes. Using collective choice theory, I show that liberalism has adopted two distinct kinds of Archimedean points reflecting different and unjustified presuppositions about the true institutional relation between politics and society. Liberalism's Archimedean search is not merely unsuccessful but biased in a way that is significant even for positions critical of liberalism. It is possible, I argue, to have a normative political theory that avoids an asocial conception of individuals without falling victim to liberalism's specific biases concerning institutional relations. The implications for both Rawlsian- or Nozickian-style liberalism are discussed, including the possibility of a political philosophy that avoids their "analytical extremism."

Liberalism is associated with the desire—and the effort—to make comprehensive normative judgments. No society or social outcome, according to liberalism, should be immune to critical inspection or accepted simply because it exists. Ultimately, say liberals, these assessments must be made by a society's own members. Yet how can individuals judge the societies in which they are embedded? How can societies judge themselves when the procedures for doing so are already institutionalized? Liberalism apparently is committed to finding an analytically external standpoint from which to evaluate society as a whole.

In recent years both the desire and the effort to secure this Archimedean point have come under increasingly heavy scrutiny. A socially disconnected judge of society, critics caution, is neither possible nor desirable (e.g., MacIntyre 1981; Sandel 1982). The identity of individuals is too bound up with the social circumstances they wish to evaluate. Therefore, human beings worthy of the name should not be asked to detach themselves from their social context even when judging it. We have met the Archimedean point and it is us.

This criticism, as one might expect, has not silenced liberalism's defenders. Some associate the Archimedean search only with specific liberal theorists rather than liberalism per se.[1] Others deny there is any deep metaphysical intent behind the Archimedean search, explaining that socially constituted individuals who are the product of a liberal democratic heritage can be sufficiently critical and politically imaginative to satisfy liberal requirements (see, e.g., Rawls 1980, 1985; Gutmann 1985). The Archimedean point, they argue, expresses, rather than denies, the importance of social constitution and need only encompass basic societal institutions rather than meddle in every area of moral, religious, or social concern (Rawls 1987).

Liberalism's critics believe it has illicitly detached individuals from their social environment; liberalism's defenders either deny this or believe this critical distance is philosophically legitimate. Both sides seem to assume that liberalism of Archimedean stripe has in a very real sense succeeded in characterizing a detached relation between individuals and society, inappropriate or distorting though this might be. Yet if the critics are right about the social constitution of people (which in the end I think they are), the ostensible detachment liberalism fashions should inevitably reflect a particular relation between the individual and society. If these critics are right, the contextual side of liberalism's supposed Archimedean point cannot be as nondescript and plastic as it seems. A specific understanding of individual-societal relations should already be inscribed in the liberal stance.[2]

This, at any rate, is my thesis. Liberalism's Archimedean points—there is not just one—reflect a certain institutionally defined power relation between individuals and the society they are expected to judge. Those who pursue the Archimedean point need not claim for themselves or others the actual power to implement whatever critical conclusions they reach. The Archimedean standpoint is hypothetical and, for most people, entirely counterfactual. Still, implicit in the Archimedean point must be a kind of command or control vis-à-vis society that one can seriously imagine having. One adopts the Archimedean standpoint to judge things that can be changed, things legitimately represented as objects of choice. To assume it is possible to alter human affairs, after all, is not the same as challenging the laws of physics or the rules governing arithmetic.

Though not always directly obtainable in reality, then, the control implied by the Archimedean point must be realistic enough to be conceivable. The Archimedean stance is not, "If we were God" but, more modestly, "If we ruled our society, or perhaps some part of it." This stance, I will argue, does not present us with an ex nihilo choice before all choices. Liberalism's Archimedean search contains an institutionally loaded notion of control that distinguishes it from empty speculation or institutionally innocent detachment from society.[3]

This institutional dimension is a liability, of course, only if one assumes the necessity of an Archimedean point. If the critics (and some defenders) are right, the idea of extreme social exile is an intellectual's fantasy we are better off without. Yet even if the Archimedean point is illusory or is simply undesirable, liberalism is not off the hook. Though its failure to attain institutional innocence is no vice, one can still ask whether liberalism introduces the right kind of institutional presuppositions.

Each of liberalism's two main variants for our purposes—roughly, classical and modern liberalism—gives an *analytically* privileged status to a different institutional relation, one associated with the market, broadly construed, one associated with politics.[4] The other relation is treated as analytically derivative. Some (e.g., Dworkin 1985, 181–204) have argued that differences between the two variants represent minor variations on a common normative theme. I think they represent significant variations on a common analytic theme—the attempt to fashion an Archimedean point. In both cases, institutional bias is the fallout from that attempt. As a result, ostensibly Archimedean definitions and analytic devices misleadingly substitute for explicit normative arguments.

What we need is an analytically equal relation between individuals and these two kinds of institutions in the sense that we no longer conceive the "real" relation as either necessarily market-based or necessarily political. As we will see, it is possible to recognize both types—and, by extension, the rich diversity of actual cases—as analytically sui generis. I will conclude by suggesting some useful implications of this analytic equality for both sides of the debate over liberalism. I will suggest, in particular, that our avoidance of analytic extremism allows us to understand more clearly why the normative extremism of some liberal theorists is not only pragmatically dubious but unnecessary on theoretical grounds.

Theoretical Tools

Any student of liberalism quickly comes to appreciate the complexity of this family of ideas involving liberty, rights, equality and political accountability. Though one can imagine a coherent set of sufficient conditions for being a liberal, this complexity seems to put agreement on the necessary conditions beyond our grasp. Yet I do believe that characteristically, if not exclusively, associated with liberalism there is a comprehensiveness of normative vision, a thoroughly anticonservative presumption that in our assessments everything, every normative verdict and every social circumstance, in principle is on the table. Arguing against Michael Walzer, Dworkin (1985, 214–20) gives eloquent expression to this idea of "struggling . . . toward generality": "We cannot leave justice to convention and anecdote" (pp. 219–20).

In order to fix ideas, I first want to focus on collective choice theory—the formal theory of collective decision making by rational individuals (see, e.g., Plott 1976)—as a working embodiment of liberal theory. Crucial to our concerns is that collective choice theory takes the entire structure of society as its proper subject matter and thereby assumes the burdens of its philosophical mentor. Yet collective choice theory brings with it new and welcome resources of formal clarity and increased systematic precision.[5] Though arguably some subtlety is lost, increased clarity about the Archimedean strategy is, in this case, sufficient compensation, particularly when the very identity of liberalism and the distinction between politics and society are unclear. Collective choice theory has proved to be an ideal vehicle for answering the challenge inherited from (one view of) liberalism, namely, the challenge to specify an external vantage point from which the appropriate evaluation of society can be made.

For the same reasons, collective choice theory is also an ideal vehicle for documenting liberalism's failure to establish this Archimedean point. The theory has two main variants, which parallel classical and modern liberalism. Each, we will see, builds into its analytical apparatus distinctive assumptions about the institutional relation between individuals and society. Neither establishes the desired analytical distance between them, or introduces neutral institutional presuppositions.

Another way to say this is that standard collective choice theory is politically biased. I do not mean the crude, though common, notion that the theory promotes one particular ideology. The evidence is now clear that collective choice theory can be pursued by those left of center (liberals in the modern sense, e.g., Arrow 1984, 303–18)—even far left of center (e.g., Roemer 1982)—as well as those right of center (e.g., Buchanan and Tullock 1962). Standard collective choice theory is neither intrinsically right-leaning nor intrinsically left-leaning.

What I do claim is that the standard theory intrinsically *leans* (one way or the other). Its basic structure either analytically subordinates politics to the rest of society (right-leaning) or analytically subordinates society to politics (left-leaning). What it does not do is give politics and the rest of society analytically equal status. In this sense it is biased.[6] Conceptually speaking, both kinds of bias are by-products of collective choice theory's attempt to establish an Archimedean point for social judgments.

The subordination of politics to society is the upshot of collective choice theory's first variant, public choice theory. Public choice theory attempts to provide an extrasocietal grounding for ethical or welfare judgments by deriving them from the hypothetical preferences of individuals inhabiting the state of nature. The latter is a conceptual device designed to capture the notion that ethical evaluations are properly made of social institutions as a whole (see, e.g., Buchanan and Tullock 1962; Nozick 1974). Judgments emerging out of the state of nature are thus understood to be presocietal or, more analytically, nonsocietal.

The subordination of society to politics is the upshot of collective choice theory's other variant, social choice theory. Social choice theory tries to find an Archimedean point for its judgments by perching "above" the societies it surveys. Every social outcome is seen as the product of a social decision process, which social choice theory seeks to evaluate. From this overarching perspective, each collective process is judged "externally" by the extent to which it satisfies certain ethically attractive criteria, for example, being nondictatorial. The resulting evaluation subsumes the entire social structure insofar as it encompasses all of society's component processes. Politics and markets, in particular, are both "regarded as special cases of the more general category of collective choice" (Arrow 1963, 5).

Both sides are aware of their differences and have not hesitated to express them. So it is not difficult to find mutual criticisms. Yet their never-questioned liberalism leads each to think an attack on the other automatically shows to its own advantage. My most striking conclusion, therefore, is not that these positions are vulnerable. It is the demonstration that the objections raised by *both* sides are correct.

Public choice theory and social choice theory are equally wrong. If my argument in these sections is sound, a third party, that is, a position that takes existing institutions more seriously than either of these variants, is the ultimate beneficiary of this family quarrel. We can conclude this, moreover, without artificially imposing alternative terms of debate or assumptions on the two combatants.

In sum, though collective choice theory was influenced by liberalism, it can return the favor. I will use it to argue that recognition of people's social nature is entirely consistent with treating existing institutions, social and political, as analytically equal. The idea, again, is not to eliminate institutional presuppositions from the analysis but to neutralize the political biases that previously accompanied them.

Finally, I will relate this reformulation to the broader philosophical concerns about liberalism. The unrealistic purity associated with much liberal theory, it turns out, is less an automatic consequence of the theory's philosophical seriousness than of its Archimedean ambitions. Reconciling normative ideals with reality does not necessarily mean weakening the ideals.

Public Choice Theory

Public choice theory assigns the Archimedean point a causal role insofar as the theory envisions the outright creation of political institutions from a setting that is analytically prior to the existence of society. This extrasocietal platform is commonly known as the state of nature. This approach, I will show, is institutionally laden despite its naturalistic bent. My point is not the by-now stale conclusion that the classic state-of-nature theory has an ax to grind. Rather, I claim that at its core, public choice theory succumbs to the Archimedean temptation in a biased form. This bias represents one finale for the untenable, deinstitutionalized approach to political theory prominently associated with liberalism's Archimedean project.

Public choice theorists, notably Buchanan and Tullock (1962) and Buchanan (1975), do not regard the state of nature as synonymous with chaos. Even in this setting people are expected to reach a kind of dynamic equilibrium consistent with a measure of control over property and endowments. This state of nature defines the baseline against which individuals assess the value of creating political institutions.

Buchanan and Tullock's state-of-nature approach, however, does not simply provide a historical scenario for the initial emergence of politics. It purports to represent the underlying relation between any political institution and its participants, whether the relation is primordial or ongoing. The state of nature is thus the extrasocietal Archimedean point against which participants and observers judge whether existing political institutions should persist. Most important, since the results of the state of nature are always available by default, these theorists argue, any movement from what Buchanan (1975, 79) calls anarchistic equilibrium must be by unanimous consent.[7] Any other rule, they suggest, would allow one group to dominate over another and therefore is already political (Buchanan 1975, 82–83).

The obvious objection is that for those living in a world already containing markets and politics, the baseline for making evaluations of politics based on unanimous consent is no longer institutionally innocent. Each participant's decisions about politics as a whole reflect the extrapolitical rights individuals can claim, rights broadly associated with, or at least mediated by, markets.[8] The state of nature's modus operandi cannot be imported into an institutional environment without acquiring institutional baggage.

Public choice theorists respond that the market is similarly subject to a detached evaluation based on the individual's prospects in the state of nature.[9] This, however, is not a simple empirical observation about the frailty of any particular institutional order, whether market or not. Rather, the underlying claim is that all institutions are necessarily fragile since they exist only on the sufferance of their participants. You can take individuals out of the state of nature, on this view, but you can't take the state of nature out of individuals.

Yet this attempt to make institutions look specially vulnerable incorrectly assumes there is an analytic asymmetry between institutional order and disorder. For the idea that reversion to the state of nature always haunts any existing institution is no more significant, as an analytic point, than is the idea that the possible emergence of political institutions always shadows any state of nature. The existing institutional framework, not the state of nature, is now the background against which social judgments, and the power to change, are determined. The historical priority of the natural setting means nothing when political and social institutions are part of the status quo.[10]

As a reminder, my target is not the alleged "bourgeois bias" of state-of-nature theory per se (see Grafstein 1987) but the specific institutional content it acquires when linked to the decision rule of unanimous consent. This link allows the state of nature to mimic the structure of genuine markets (see Buchanan 1987, 1435; Coleman 1988, 243–76). As a result, say public choice theorists, the polity emerging from the state of nature is shown to be an efficient outcome in the sense that, and for the same reasons that, the outcomes of perfectly competitive markets are efficient. But the addition of unanimous consent gives the state of nature a distinctly institutional cast.

In defense of this link, public choice theorists might argue that nature itself is not institutionally neutral. Since nature currently allots eyeballs, talent, and other socially useful things, political redistributions are impossible or very costly. Notwithstanding the possibility that others may coercively use your brain power, for example, your brain itself is yours in markets and politics alike. Also, nature helps determine precisely who experiences particular pleasures and pains. This will preclude major political redefinitions not only of what constitutes donation but of who is the "donor."[11] Together these practical considerations suggest that property is naturally individualistic in a way no institution can fully negate. (For nature's influence see, e.g., Hirshleifer 1987, 194–210).

While I would concede the (increasingly) limited validity of this point about "natural property," I want to ward off a mistaken impression it may reinforce. The plausible link between natural attributes and a necessary degree of individual property should be carefully distinguished from public choice theory's proposed link between individual property and unanimous consent, which is an institutional mechanism (Dworkin [1981], e.g., concedes these natural limitations without flinching from the principle of collective control of natural assets). In tandem, these links might lead one to think the unanimous consent rule is not merely biased toward the status quo but invariably justifies a conservative view of property. This is a serious misunderstanding of the rule. Unanimous consent has this function only within institutional settings in which individual property rights are already established.

Rawls (1971) in fact cleverly exploits this distinction between the unanimous consent rule and the kind of property the rule oversees. He posits the well-known original position from which individuals ignorant of any socially relevant facts about themselves select the basic principles that will govern the institutions of their society. On the one hand, these bird's-eye decision makers select principles by unanimous consent, the classic instrument of social contract theory. On the other hand, Rawls requires participants to make their selection impartially behind a veil of ignorance concerning who has or owns what. As a result, the original position combines public choice theory's favorite decision rule for foundings with the presumption that property, resources, and personal endowments are ex ante collective assets whose principles of allocation are to be chosen by the group (Hare 1974; Nozick 1974, 183–231; Sandel 1982, 66–103).

Public choice theorists are still free to maintain that "only through the securing of unanimity can any change be judged desirable on the acceptance of the individualistic ethic" (Buchanan and Tullock 1962, 261). This ethic, however, is determined by—as much as it determines—a view of the proper relation between politics and the rest of society. To demand a better anchor than this is to embark on a hopeless search for preinstitutional actors in an institutional world.

Social Choice Theory

From the standpoint of social choice theory, any social outcome can be understood as a collective choice, the result of society's particular way of aggregating the preferences of its members. Social outcomes, after all, do not occur by magic. This comprehensive view of social processes explains why social choice theorists tend to question whether unanimous consent, the darling of social contract theorists, can be considered a true decision rule. Since this rule accepts the status quo as the undecided outcome whenever decision makers fail to reach unanimity, social choice theorists argue, the rule does not adequately characterize the actual genesis of the status quo. This interpretation, we will see, is symptomatic of social choice theory's larger failure to recognize the inadequacy of the alternative Archimedean point it posits.

Consider a decision whether to depart from the status quo *S* to a new state *N*. If *S* is preferred by some affected party, when all others prefer *N*, under a unanimous consent rule *S* is the social outcome. Public choice theorists would say that the status quo remains in force not because it was chosen but because the alternative was not chosen. Yet contrary to appearances, answer critics of unanimous consent, *S* does not persist simply by default. It must, by their definition, represent a particular aggregation of individual preferences. What has happened, in fact, is that the individual who exercised the veto has decided for all. This means the rule of unanimous consent becomes its supposed opposite, dictatorship. To put this criticism more sharply, "unanimous universal consent has the logical form of the square circle" (Rae 1975, 1282).

In a nutshell, the social choice theorist's claim is this: "Abstention from a decision cannot exist; some social state will prevail" (Arrow 1963, 118). Or, in the explicitly political terms of scholars who are not social choice theorists, "government *inaction* is as much a choice of policy as government action" (Dahl and Lindblom 1953, 338). But this interpretation of nondecisions will not convince defenders of the unanimous consent rule. Granted, the argument against unanimous consent has the unimpeachable form of a reductio ad absurdum. Yet those who defend unanimous consent as a meaningful decision rule can surely construe this reductio to their own purposes. Applying their view, the reductio would show not the absurdity of universal unanimous consent but the absurdity of equating outcomes reached by default, by nondecision, with dictatorial decisions.[12] The fact that some social state like *S* prevails does not imply that what prevails was chosen (see Buchanan 1975, 78).

Whether this non sequitur charge is correct hinges on one's definition of a political decision. Rather than monger semantic intuitions, I will note, first of all, the intimate connection between this formal attack on unanimous consent and a particular conception of politics. To hold that every possible social outcome emerging from the application of this rule is the result of an implicit or explicit political decision is to hold that every possible outcome is necessarily within the purview of that decision-making process. In particular, society cannot, in the view of social choice theory, simply not choose *N*. It must, as a result, choose not-*N*, that is to say, *S*.

This social choice interpretation, in sum, presupposes a particular conception of collective decision making and its authority over the status quo. It is a conception under which the uncontroversial observation that there is always a social outcome ("some social state will prevail") implies the more interesting claim that the existing collective choice mechanism (implicitly) produced it. The claim, in other words, is that omissions are all *acts* of omission.

Why is the failure to make a collective decision a collective decision? An answer one often reads is that a social decision takes place every time society changes or fails to change the prevailing social state. There is, we are told, nothing automatic about the status quo. Under this presumption, for example, every wealth profile for a society results from a mechanism for allocating wealth: "A society must have some social product over any interval of time, and [therefore?] must have some set of rules for its allocation" (Rae 1975, 1278).[13]

Yet why should a failure of these rules to operate be equated with a decision made within them? Social choice theory's answer repeats the original reasoning about nondecisions at a higher level of abstraction: the failure to change a set of rules is a political decision not to change them. Similarly, the continuing existence of a political mechanism—nondecisions and all—represents a social choice in favor of that mechanism and its consequences. At least, "such valuation . . . seems to be implicit in every stable political structure" (Arrow 1963, 90). Social choice within the unanimous consent rule, in this view, is implicitly a collective choice of that rule and the consequences of using it.

What keeps this entire line of reasoning afloat is a plausible intuition. Political decision rules are not miracles but emerge out of some comprehensible collective choice mechanism. Such rules, therefore, must be understood as the product of a social decision, a product that in principle can be recalled. To social theorists, we have seen, it is simply a misunderstanding to imagine a social outcome that cannot be understood as the (implicit) product of a decision but that simply is.

Defenders of the unanimous consent rule can respond that without some restriction on what qualifies as a collective choice mechanism, social choice theory merely restates a very general philosophical claim: social outcomes are socially determined. Yet this claim, valid or not, begs the question at issue. For to argue, as social choice theorists apparently must, that every relevant political state of affairs results from a collective decision is to blur a crucial distinction between the social determination of social outcomes and the choice of social and political outcomes. The two are not equivalent. The emergence or persistence of a political decision rule may be socially determined without being an object of collective choice, that is, the product of an actual institutional arrangement for making choices about social and political outcomes.[14]

If every social outcome or status quo is assumed to be the (tacit) result of a social decision, so that neither can exist merely by default, the social decision in question must be political. Only politics conceivably has the scope to evaluate every social element of society including the elements the participants bring to the decision process. Social choice theory's verdict on unanimous consent, in other words, goes beyond a formal ruling on how to categorize nondecisions. It presupposes a deeper understanding of the nature of this or any collective choice mechanism and its relation to the rest of society.

To say we choose not-*N* whenever we do not choose *N* is, in short, to say the status quo is never really behind us but is always in front. In principle, it is an object of decision just because it is a social condition of decision. To treat nondecisions as equivalent to decisions is thus to confirm the voraciousness of social choice theory's conception of collective choice. Under this variant, the social institutions we have depend on the politics we have; likewise the politics we have depends on our collective choice, on the (tacit) decisions we make.

Yet it is one thing to recognize that within a specific institutional framework—the unanimous consent rule—the difference between society's being in *N* or *S* is determined, as in our example, by the preference of one participant, given everyone else's preferences. It is quite another thing to conclude that this rule allowed the individual to decide *S* for society (see Buchanan and Tullock 1962, 260). One needs to consider in detail what constitutes a decision relative to the specific political mechanism involved and what constitutes a choice of that mechanism. As we have seen, it is just intellectual confusion to suppose that every such social choice mechanism can and must be understood as the product of a social choice mechanism.

Liberalism's Principled Judgments

For public choice theory, individuals ultimately exercise control over their circumstances through the threat of exit, vetoing arrangements they dislike. For social choice theory, the way to ensure proper control is to design institutions properly, which is a political activity. Perhaps, liberals might complain, our focus on control says less about liberalism than about the way collective choice theory happens to formulate its Archimedean points. The values of liberalism, they might say, are not predicated on the power of observers or participants to effect or prevent a (re)designing of society. Liberalism is a deeper philosophy offering an argument from principle.

Historically, liberalism strongly associates power or control with rights, broadly understood. So in evaluating the liberal response, it may be helpful to focus on these. Specifically, I will start by demonstrating the institutional bias built into social choice theory's understanding of rights and then show that this bias survives an attempt to reinterpret this understanding in terms of principles.

How does social choice theory recognize rights? A standard way is to use respect for rights as one of the criteria for assessing or designing the relevant collective choice mechanism. A rights-based criterion, for example, could require that social choice processes accord to each individual areas of choice over which he or she is decisive. Decisions emerging from social choice processes, for instance, would have to be consistent with preferences of individuals regarding their religious beliefs.

At first sight this technique for incorporating rights seems quite reasonable and, in any case, hardly worthy of ideological suspicion. Yet critics have demonstrated that individual rights are, in fact, integrated into social choice theory in a biased way. The extent to which social choice theory's handling of rights falls short of analytic neutrality is nicely brought out in its analysis of an apparent conflict between otherwise natural liberal principles.

Consider two attractive criteria that might jointly govern a liberal polity. One is the rights principle just discussed. The other is social choice theory's version of the familiar unanimity principle, prescribing the rejection of an alternative if everyone in society prefers some other one. In conjunction, Sen (1982) discovered, these two reasonable principles can make it impossible for society to determine a best outcome. Every outcome is judged inferior to another, which means society cannot arrive at a settled conclusion.

This conflict, however, is an artifact of Sen's formulation of the rights principle, a formulation rooted in social choice theory's conception of collective choice (see Sugden 1981). On Sen's reading, the two principles are applied to the decision process simultaneously, so to speak. On a *public* choice reading, in contrast, individual rights are not properly components of collective choice but are the foundation on which social processes operate. The influence of individual rights on society, in other words, should be exerted prior to any collective decision.

Social choice theory eventually excludes outcomes that are inconsistent with the preferences of individuals having rights over the matter. According to public choice theory, these outcomes should not have been in the domain of social decision making in the first place. And if they are excluded before the actual social choice is determined, the unanimity principle never gets a chance to bump up against the rights principle, which has already had its effect.

This, say public choice theorists, more

accurately represents the nature of rights and precludes the conflict. "If I have a right to choose to live in New York or Massachusetts," writes Nozick (1974, 166), "and I choose Massachusetts, then alternatives involving my living in New York are not appropriate objects to be entered in a social ordering." These precluded options do not belong in the pool even as possibilities. In the public choice formulation, then, at least some possible social outcomes are external to collective decision making, whereas in Sen's social choice formulation all possible outcomes are internal.[15]

Public choice theory is often seen as the more ideologically closed system. Its derogation of Sen's discovery suggests that the greater openness of social choice theory to alternative criteria for evaluating societies is somewhat illusory. As one more example, consider property rights instead of freedom of thought. In the spirit of openness, defenders of social choice theory will be quick to emphasize that respect for property rights can be incorporated as one of the criteria for assessing particular collective choice mechanisms. Social choice theory is free to prescribe the choice of a market mechanism and its associated rights if these receive a positive evaluation.

Fair enough. Social choice theory is biased toward socialism only insofar as social choice theorists choose to apply socialist criteria. Still, social choice theory's ostensibly bird's-eye view of society implies a political mechanism that renders the status quo an object of social evaluation. Individual property is one aspect of the status quo; so it, too, must first be analytically collectivized, that is, subject to collective assessment. The crucial point, then, is that property is first analytically detached from individuals even if it is eventually reattached. Only then is its private status justified in competition with other values.

With this as background, we are in a position to consider the argument raised above that my institutional analysis overlooks the ethical core of the liberal position. Taking this line of defense, Sen (1982, 306–7) notes that a social ordering of possible social outcomes can be interpreted institutionally, as I have, or as reflecting an ethical judgment about social welfare distinct from the specific social mechanisms producing it. Even if one wants to think of rights as outside collective choice institutions, he observes, one presumably would still want to make a principled, overarching judgment in favor of societies recognizing rights.[16] Nozick's complaint, Sen argues, ignores the latter use of social choice theory.

The latter use, unfortunately, presupposes an institutionally innocent framework for surveying the comings and goings of actual societies. In truth, principled judgments about society are inevitably judgments about the propriety of its underlying institutional arrangements. These judgments cannot pretend to incorporate institution-neutral conceptions of property or individual rights. Either Sen's social choice assumption, Nozick's public choice assumption, or a position somehow synthesizing both, must inform the observer's judgment.[17]

Sen's social choice assumption, in other words, does not follow from Sen's argument against Nozick; it provides its foundation. Sen (1982, 306) writes, "But one can also argue that, if I believe that it is a better society which—given other things—lets Nozick decide where he wishes to live, then I must *assert* that it is socially *better* that Nozick should be permitted to live in Massachusetts as desired by him." Yet who exactly does the permitting and why should the issue be posed in those terms? Choosing the lesser of two evils, Nozick would acknowledge it is a better society that permits freedom of residence. A still better society, he would argue, does not presume it is society's role to grant permission in the first place.[18]

Restoration of Balance

I do not argue against Sen's approach to champion Nozick's. Neither conception of (property) rights, understood as a type of effective control, is conceptually more valid than the other. Both presuppose an institutional framework in which the asserted rights are defined. One conception, Nozick's, is appropriate to the market; and the other, Sen's, is appropriate to the political arena. They ought to be seen as analytically equal. Individual property is not, conceptually speaking, the "real" base for political property; collective property is not the "real" source of private property. In liberal democracies, in other words, we recognize that individuals negotiate between institutional arenas in which they have property in both senses, neither necessarily subordinate to the other.

Analytic equality, needless to say, is not the same as normative equality. Its restoration does not automatically resolve questions about justice, the good, or the best political arrangement. It does force proponents of the alternative answers, left and right, to argue on the basis of values and principles rather than a conceptual sleight of hand. In judging societies, we forgo the intellectual manipulation that substitutes institutionally loaded definitions of property for arguments about the normative merits of each kind.

When, in contrast, all property is simply defined as ex ante public, one is encouraged to see individual property as a tax loophole or at least as in particular need of justification. When all property is simply defined as ex ante private, there is a parallel tendency to see any societal claim as a form of theft. Though these definitions of property do not automatically translate into political extremism, they do foster it.

From these extremist perspectives, in fact, the classic U.S. normative standard, "equality of opportunity," can look like a pragmatic compromise between liberal and conservative principles.[19] To a modern liberal, for example, this formula, or at least its implementation, is hopelessly inadequate, since it does not fully address inherited biological, financial, and cultural differences. To the conservative, whose stand is a mirror image, the idea of equal opportunity is merely a way station to socialism. In order to assure true equality of opportunity in society's race, we would have to arrange new heats continually. As a consequence, whatever differential rewards emerged from one heat could never be allowed to matter.

On either view, equal opportunity is little more than a political compromise, a truce engineered by a society unable to live up to the normative consequences of its own principles. Rather than a failure of will, however, equal opportunity may represent the proper balance between society's social and political institutions, that is to say, between the kinds of control over circumstances each embodies, either in the form of civil or political rights.

From this bi-institutional perspective, equality of opportunity is its "compromised" version *might* be judged a remarkable property settlement that precludes, rather than marks, a divorce between individual and society.[20] According to this "compromise," the proper relation between individual and society incorporates distinct though essential kinds of property rights, narrow and broad. These rights allow us to pursue and enjoy the fruits of our plans while providing some measure of influence over the larger social forces affecting our chances of success.[21]

Put more generally, each individual has an opportunity to have a meaningful and self-directed life. Yet this autonomy, in principle, must not be accompanied by—or at least must not be the cause of—unreasonable deprivations of other people's opportunities. Equal opportunity, moreover, recognizes that the benefits of individual freedom are offset by the cumulative externalities resulting from the decisions of others. It also recognizes that these externalities can supplement freedom, even with regard to property rights in the narrow sense. Other people enhance "our" marginal productivity and worth. So equality of opportunity balances the rights of the marketplace with the control over one's circumstances embodied in political rights.[22]

Confronting the inaccessibility of the two ethical extremes, Suppes (1988, 90) writes, "Too much discussion of matters of justice and equity has centered on unrealistic ideas such as those of Rawls concerning the initial distribution under the veil of ignorance, or, to take a quite different example, Nozick's idealized and equally unrealistic concept of entitlement. . . . We are always rebuilding the equity ship at sea. We are not going to run ourselves aground and start afresh." Yet the reasons for this are not just pragmatic.[23] The normative sensibility of these extremes is unrealistically a-institutional. When we have rights, we assure our control in different institutional settings, not over some homogeneous thing called "property" but over property defined in different ways.[24]

Sandel versus Rawls

One of the "extremists," Rawls (1971), opts for a collective interpretation of endowments. According to Sandel (1982), this hypothetical disengagement from existing attachments, tangible and intangible, requires an asocial and therefore incorrect conception of individuals in the original position. Yet in truth, Rawls (post-1971?) does recognize that the individuals who populate the original position are not ahistorical but reflect "a conception of the person implicitly affirmed" within "a democratic society under modern conditions" (1980, 518). He appreciates that the way persons in the original position are conceived depends on certain socially influenced views of personhood. The original position reflects "a political conception of justice," as he later puts it (1985, 224) when responding to Sandel.

From our institutional perspective, on the other hand, Rawls's renunciation of the metaphysics of personhood does not go far enough. Like his critics, he continues to see the role of these constitutive social forces in personal, even psychological terms. On an institutional view, the informational constraints imposed on individuals in the original position also serve, in effect, to superordinate political institutions over social institutions.

For as we noted earlier, the original position is a political institution by virtue of the way it collectivizes property and endowments. It accomplishes this by strictly limiting the impact of socioeconomic relations on political decisions and thereby presupposes a particular balance of power between political and social institutions. In this sense, the veil of ignorance is more an institutional than a psychological feature of the social contract.[25] Understood this way, the role of the original position is to assure not impartial choice so much as a proper balance between two sorts of institutional influence.

A similar point can be directed at Sandel (1982). He rightly objects to the Archimedean idea of a radically disembodied self detached from its social roles. The self's bootstraps, indeed, are not that strong. Yet many of the characteristics liberals impute to this self can be interpreted in institutional terms. On this view, the self does not rely solely on its own powers when detaching itself from the social roles it embodies; it detaches itself by virtue of its political, as opposed to social, roles. Oddly enough, Sandel's indictment of the disembodied liberal self may overlook the specific effect of its embodiment in particular political settings.

Insofar as politics institutionalizes our detachment from our ordinary social environment, in short, this detachment does not contradict Sandel's thesis that we are socially constituted. Sandel's *philosophical* position thus excludes less of liberal theory and its respect for politically institutionalized detachment than one might first imagine (see Care 1985, 465; Gill 1986).

Whereas Sandel insists on the social constitution of the self, I envision what might be termed the sociopolitical constitution of the self. If this broader idea is correct, the critical abilities to which liberals appeal are not just an individual achievement but testimony to the expansion of possibilities associated with politics. This does not mean the self can detach itself from society in one synoptic act. But perhaps like Theseus' boat, the self can be significantly reconstituted over time, piece by piece and in a politically conscious way. We may get the society we deserve, but politics determines, in part, how deserving we are.

Concluding Remarks

The preceding has highlighted the institutional presuppositions hounding the liberal attempt to theorize within an extrasocietal framework. The mere presence of institutional presuppositions is not the problem. What causes the difficulty, in fact, is that the institutional context is not fully represented in liberal theory. Political bias is the inevitable result. The solution is to acknowledge that society's political and market institutions are as much a part of the status quo as the individual participants themselves. In this light, what one might call liberal liberals and conservative liberals primarily disagree about the proper balance between political institutions and institutions of the rest of society.

As both sides must recognize, people typically lead their lives and shape their social environment within institutions. Like it or not, institutions are the vehicles that happen to be around. Less obvious, the baseline for exercising this control—which establishes the contrast between what people currently have and what they would like to have and between who they currently are and who they would like to be—has an institutional dimension. People, no less than normative theory, must choose without an Archimedean point. Once this is conceded, much of the theoretical apparatus of standard liberalism can be reintroduced if desired.

For example, though it bifurcates the notion of property, the institutional perspective does not necessarily abjure appeals to individual property rights. But the rights thus ascribed do not reflect any *ontological* inequality between individuals and the societies they inhabit (see Grafstein 1988). Alternatively, one can cleave to Rawls's original position by reinterpreting it as a mechanism designed to help sort out the proper structural relation between politics and other parts of society. Which resources and property are suitable for pooling in the original position and which are not would still have to be decided by whatever moral standards are deemed appropriate. Though we appreciate the role of institutions in determining the types of control we can have over our own lives and the societal conditions influencing us, it still requires normative reflection to decide how to keep these types of control in proper balance.

Notes

I thank George Klosko and Kristen Monroe for their comments on an earlier version of this paper presented at the 1989 annual meeting of the Midwest Political Science Association, Chicago.

1. See the useful essays and introduction in Damico 1986.

2. Claims to the effect that liberalism's descriptions of people are socially loaded abound (e.g., Macpherson 1962). As will become clear, my claim is that the very relation of detachment reflects a particular sociopolitical context.

3. Compare, e.g., Unger's (1987) more far-reaching "ultratheory," which seeks the actual (and continual) remaking of society.

4. The terms *classical* and *modern* are only suggestive. Contractarians of a Lockean type (at least according to one received image of Locke) are, for example, classical, whereas Ackerman's (1980) neutrality-oriented liberalism is modern insofar as it envisions a dialogue over the distribution of initially pooled resources ("manna").

5. For an analysis of classical liberals using the techniques of collective choice theory see, e.g., Taylor 1987.

6. Some see a kind of bias in the way collective choice theory uses the idea of preference aggregation to model the types of judgment and dialogue involved in collective decisions (see, e.g., Elster 1986). This charge, which is tangential to our own specific discussion, is disputed by Sen (1986, 232-37). For related allegations of bias see Frohock 1987 and, in response, Grafstein 1989.

7. As an exegetic point, Buchanan and Tullock (1962) focus on uncertainty about the future as the basis for explaining the rights embodied in the unanimous consent of the state of nature, whereas Buchanan (1975) emphasizes the equilibrium dimension.

8. This assumes a certain alignment between preferences and entitlements; see Green 1982 and Coleman 1988 (pp. 281-87).

9. As Brennan and Buchanan (1985, 26), in fact, make clear, political order is prior to fully functioning markets.

10. It is not my intention to dismiss out of hand claims about the normative importance of historical scenarios concerning the creation of institutions or the appropriation and transfer of property. My point, rather, is that these normative claims are too often conflated with the particular analytic mechanisms used to dissect them. The state of nature—used for both historical and analytical purposes—is an example of this conflation, which becomes particularly noticeable when this analytic scheme is made to confront historical details and the complications of intergenerational transfers (see Litan 1977; Steiner 1977).

11. I am hesitant to leave this testimony to the power of nature unqualified, given the well-respected view that the individual self is socially constituted—an idea to which I am not exactly hostile. Public choice theorists would still be pretty safe, I think, to hold that certain facts about human nature may limit the possibilities for social change, even if the social significance of these facts is historically variable.

12. Under a narrow interpretation, Rae (1975) merely argues it is impossible, in general, to ensure explicit unanimous consent for every prevailing social state. His discussion and examples, however, warrant a broader and less trivial interpretation according to which nondecisions are the same as decisions. This interpretation, in any case, is the one most consonant with the social choice approach, our main interest.

13. Rae (1975, 1278-79) also suggests as an example the principle that actions not specifically illegal are legal. This, however, relies on a deeper principle prohibiting ex post facto criminalization. Assuming this principle logically necessary vitiates some fundamental debates over the Nuremberg trials. Millikan's (1984, 61-62) analysis of the parallel case of belief illustrates what is questionable about the whole thrust of this kind of reasoning. If I go to sleep, I do not believe Jack the Ripper is under my bed. From this, someone adopting the logic of social choice theory presumably would conclude that if I go to sleep, I believe the Ripper is not under my bed. Yet as Millikan notes, this conclusion does not follow; I may never even have heard of Jack the Ripper. Analogously, not choosing X is not equivalent to choosing not-X.

14. Chapman 1983 (p. 5) draws a similar distinction between social and legislative choice. Its importance is also reflected in the debate on nondecisions and political power (e.g., Bachrach and Baratz 1970; Wolfinger 1971).

15. Cf. the defense of the social choice approach by Perelli-Minetti (1977) and Gibbard (1974), although their assumption of a universal social ordering of all possible alternatives comes pretty close to begging the question posed here. Pressler (1987) believes the paradox stems from Sen's requirement of a binary social choice function (see also Sugden 1985). Pressler's argument, however, is really a temporal version of public choice theory's rights argument. He suggests, in effect, that individuals participating in social choice processes would choose game-theoretically, anticipating that some moves provoke further moves, whereas others terminate the game. In this case, the termination of the game is the counterpart of removing an alternative from the social ordering.

16. Responding to Barry's (1986) argument that the rights and unanimity principles are compatible since social preference is not at issue when an individual right is exercised, Sen (1986, 224) repeats his response noted in the text. When discussing the interpretation of social choice theory used in this response, however, Sen (1986, 216) notes that it implicitly assumes "that from every non-empty set something or other *is* chosen." It assumes, in other words, there are no nondecisions.

17. As a minor point, it is not my argument that the idea of social welfare must be operationalized in terms of social decision processes (see Arrow 1963, 106-7).

18. It is interesting to note that when arguing for the limitations of the public choice account of freedom, Sen (1983, 18-20) uses the revealing example of making hypothetical choices for an unconscious person. Incidentally, Rawls (1987, 12-15) illustrates how important it is to keep the Sen and Nozick approaches distinct. When Rawls suggests that matters

of individual liberty should simply be "taken off the political agenda," is he siding with Nozick? Perhaps not: he makes the same suggestion for guarantees of "sufficient material means to make effective use of those basic rights" (1987, 17).

19. The terms *liberal* and *conservative* are used here with their ordinary political meaning; many of the conservatives I have in mind are philosophically liberal.

20. I am struck by the comparison with Berlin's (1969) two concepts of liberty. He recommends a pluralism of values that cannot be put into a neat, hierarchical order. There is also Walzer's (1983) notion of spheres of justice, though he is much more concerned with the distinct functions performed by particular spheres than with their distinct institutional structures.

21. The relation is not just complementary but one of mutual support. Certain kinds of individuals are more likely to sustain liberal polities; and, ideally, liberal polities can encourage individuals to have the requisite "instrumental virtues" (see Galston 1988).

22. Moulin and Roemer (1989) also employ a quasi-bifurcated view but draw the line differently, allowing collective rights over the "external world" and individual rights only over the self. I say "quasi-" because, as their axioms make clear, they have not formally integrated the individual rights view I have associated with public choice (e.g., Green 1982) into their roughly social choice view of collective rights (see Moulin and Roemer 1989, 362-63).

23. For a fuller discussion of the pragmatic dimension, see Anderson 1986.

24. For those curious about the possibility of integrating this bi-institutional model into collective choice theory, I should mention that the idea behind it is not exactly foreign to the literature, which in some cases has implicitly supplemented the idea of private property with political property. It is quite consistent, e.g., with Aumann and Kurz 1977 (see also n. 22). It is similarly in the spirit of Becker (1983, 1985) and Peltzman (1976), who writes, "There is essentially a political auction in which the highest bidder receives the right to tax the wealth of everyone else" (1976, 212). Though in these models there is a concept of individual property, the ultimate budget constraint for political actors is defined by total tax revenues. By the same token, Peltzman's comment suggests an unfortunate conflation of the empirical and normative dimensions of property. In terms of effective control, taxable wealth is not yours unless it is not taxed. The notion of possession built into his comment about "the wealth of everyone else" is therefore empirically dubious. Still, the actual power of political actors to control society's money does not necessarily weaken an individual's moral claim to what is taxed.

A parallel gap between Becker (1983) and the present proposal shows up in his assumption that political agents maximize their "full income" (what agents would have if they devoted every moment exclusively to generating money). If one treats income derived from politics and the market symmetrically, full income is a function of the agent's possible choices involving both arenas. Yet as Becker (1983, 374-78) portrays it, political income is simply a net addition to, or subtraction from, full income defined independently of political activity. This either violates his own assumption of redistribution distortions induced by political subsidies or requires particularly strong assumptions about the "shadow price" of political activity (see Becker 1976, 92-93).

25. This is why I think the objection that the veil of ignorance deprives individuals of their individuality is overrated. Hampton (1980, 334) goes so far as to argue that the participants in the original position collapse into one, a conclusion she bases on the principle of the identity of indiscernibles. Yet the original position surely includes in an essential way the flesh-and-blood individuals who must live in and maintain the well-ordered society after the veil is lifted.

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EVOLUTIONARY BIOLOGY AND POLITICAL THEORY

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The traditional emphasis on human nature as the foundation of politics needs to be reexamined from the perspective of contemporary biology. Because biological processes operate independently on the individual, the social group, and the species, an evolutionary approach to both observational research and cost-benefit analysis does not entail reductionism. Selfishness and altruism, participation in social groups, languages and cultures, and the rise and fall of centralized states can all be illuminated by empirical evidence and theories in the life sciences. For political philosophy, a new "naturalism" points to a return to the Aristotelian view that values or standards of judgment have rationally intelligible foundations, thereby challenging the relativist or nihilistic orientation that has characterized most contemporary thought.

Human nature has been at the foundation of thinking about politics since the ancient Greek philosophers developed the concept of nature as we know it in the West. As political philosophy is conventionally taught and studied today, however, human nature is no longer the subject of scientific inquiry in the precise sense. Instead of formulating hypotheses and subjecting them to empirical tests, political theory in the twentieth century has generally been viewed as a study of the ideas and history of famous thinkers who wrote about human nature and politics.

The tradition of political philosophy arose and flourished in the hands of thinkers who did not make such rigid distinctions as those now practiced in our universities and our intellectual life. Plato's *Republic* presents an educational curriculum that includes the disciplines we call mathematics, physics, chemistry, and biology, as well as those we consider to be philosophic and political in character; Aristotle wrote at least as widely on matters of biology and physics as on politics or ethics. In both the Lyceum and the Academy, not to mention other ancient schools, the contemporary divisions between scholarly disciplines did not exist.

The irony of the gap between what Snow called the Two Cultures is the proliferation of scientific research that bears directly on political theory (see, e.g., Alexander 1979, 1987; Gruter and Bohannan 1983; Ruse 1986; Wilson 1975, 1978). Evolutionary biology makes possible a deeper understanding of human origins and the emergence of political institutions. Neurophysiology, neurochemistry, experimental psychology, ethology, and ecology provide empirically based information about human nature. In the last generation, the fossil record of human origins has been greatly expanded, and the mechanisms of inheritance (the structure and function of DNA) understood for the first time; and the science of social behavior among animals has been enriched by direct observation in the field, by laboratory experiment, and by theoretical models of natural selection.

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Although there is an emerging subfield in political science known as biopolitics (Corning 1986; Schubert 1989; Somit 1976; Thorson 1970; Watts 1984), the study of human nature and politics from the perspective of the life sciences has not yet become an accepted approach in any of the social sciences. It seems fair to assume that this state of affairs is not likely to survive the continued advances in the natural sciences. Over the next generation, barring nuclear war and the demise of advanced civilizations, research in the life sciences will doubtless expand our knowledge and our ability to manipulate biological phenomena. The political process must sooner or later be fundamentally affected by the power to change not only the environment but also the behavior and genetic composition of humans themselves (Blank 1981; Kass 1971).

As a result of these trends, I suggest that a "naturalist" perspective is emerging, making it possible to view human politics from a perspective consistent with both the tradition of Western political philosophy and the findings of contemporary biology (Masters 1989a). Rejecting the view that social science will be totally absorbed by (or "reduced" to) biology, I presume that human behavior is in many important respects unique in the natural world. But unlike those social scientists who have ignored biology or assumed that its introduction into the study of human behavior is ideologically motivated, my analysis seeks to overcome the gulf between scientific research and human self-awareness.[1]

A more scientific approach to political theory must address the age-old theoretical questions of human nature and the state. Biological research can illuminate our understanding of human nature by considering the foundations of human selfishness and altruism, of our participation in social groups, of human languages and cultures, and of politics itself. The origin of the centralized state can, for example, be explored by linking contemporary theories of natural selection to the study of social cooperation in political philosophy, game theory, and history (Alexander 1979; Margolis 1982; Masters 1983; Schubert 1989; White 1981). Although such an evolutionary perspective on human society has often been attacked as ideologically biased, careful analysis shows not only that evolutionary theory is consistent with a wide range of political opinions but that the denial of a natural foundation of human behavior is itself often ideologically motivated (Caplan 1978; Kaye 1986; Masters 1982).

Politics As a Biological Phenomenon

My central thesis can be summarized in simple terms. Human behavior is as open to scientific study as the behavior of any other living being. But human societies are different from social groups among nonhuman primates or other animals. Although laws, customs, and governments are a response to situations like the situations in which other animals cooperate, human language makes it possible for our political and social institutions to take on a life of their own. Each human culture is a self-perpetuating system that becomes part of the environment facing the individual. Rules that a child learns to obey have developed over time and, if lasting, have been subject to influences transcending human choice and will (Campbell 1965; Chagnon and Irons 1979; Lumsden and Wilson 1981). Since natural selection can have an impact on the origins and functioning of man-made institutions, the old academic controversy of nature versus nurture needs to be replaced by research on the complex interactions between cultural and biological factors (Alexander 1987; Cranach 1979; Gruter and Bohannan 1983).

Because human behavior has distinct

characteristics at the levels of the individual, the group, and the species as a whole, explanations of any one level cannot be deduced automatically from findings at another level. To understand human nature, it is necessary to distinguish carefully between the nature of the human individual, the nature of societies and states, and the nature of the species. This requires a careful separation of three distinct levels of analysis, each roughly corresponding to different disciplines in the social sciences: (1) the behavioral characteristics of individuals which have been the conventional focus of psychology, (2) the repertoire of human social behavior studied in child development and sociology, and (3) our species' capacity to invent and transmit culture from generation to generation—the subject matter of cultural anthropology. Findings on each level need to be integrated in the context of the neo-Darwinian, or "synthetic," theory of evolution when human politics is viewed as a biological phenomenon (Corning 1986; Masters 1975; Schubert 1989).

From the perspective of the life sciences, human beings have a complex "nature" making possible the formation of differing kinds of social systems. Like nonhuman primates, our species has a behavioral repertoire that includes both social cooperation and competition (Alexander 1979; Cranach 1979; Tiger and Fox 1971). Unlike the communicative systems of other animals, human speech makes it possible to learn, encode, and share information without primary reference to sensory cues from the physical environment. Since languages can be compared to genetic material in their capacity to store and transmit information (Masters 1970), human *cultures* seem to resemble animal *species* in their evolution and adaptation to ecological conditions. It is precisely because human behavior is largely learned and varies from one culture to another that our species' social life is understandable in the context of evolutionary biology.

Human Nature

Many traditional philosophic debates can now be illuminated if not resolved by evidence from the life sciences. In one tradition, represented by the ancient Greek Sophists, by Hobbes, and by behaviorist psychologists like Hull or Skinner, human nature is hedonistic or selfish. In this view, the individual's pleasure-pain calculus determines behavior as a function of environmental experience. In the contrary approach, epitomized by the theories of Plato (*Republic* 4.435e–444a) or of Freud, human nature is more complex, since different "parts of the soul," or psychological principles, are naturally distinct. From this perspective, social cooperation and the restraint of desire are as natural for humans as the pursuit of individual pleasure.

Contemporary research in ethology and social psychology provides a way of reconsidering this philosophical debate. Humans, like other primates, have a behavioral repertoire that includes facial displays of emotion that express and communicate cues of reassurance and bonding as well as of threat, fear, and competition. These displays have similar characteristics throughout the primate family and in many instances are expressed and decoded in a similar manner across all known human cultures (Hinde 1982; Ekman and Oster 1979; Cranach 1979; Masters et al. 1986; Montagner et al. 1988). Our experimental studies of viewers' responses to the nonverbal displays of leaders confirm that this system of communication effectively transmits emotions that can interact with cognitive attitudes to form political opinions (Lanzetta et al. 1985; McHugo et al. 1985; Masters et al. 1986; Sullivan and Masters 1988a, 1988b).

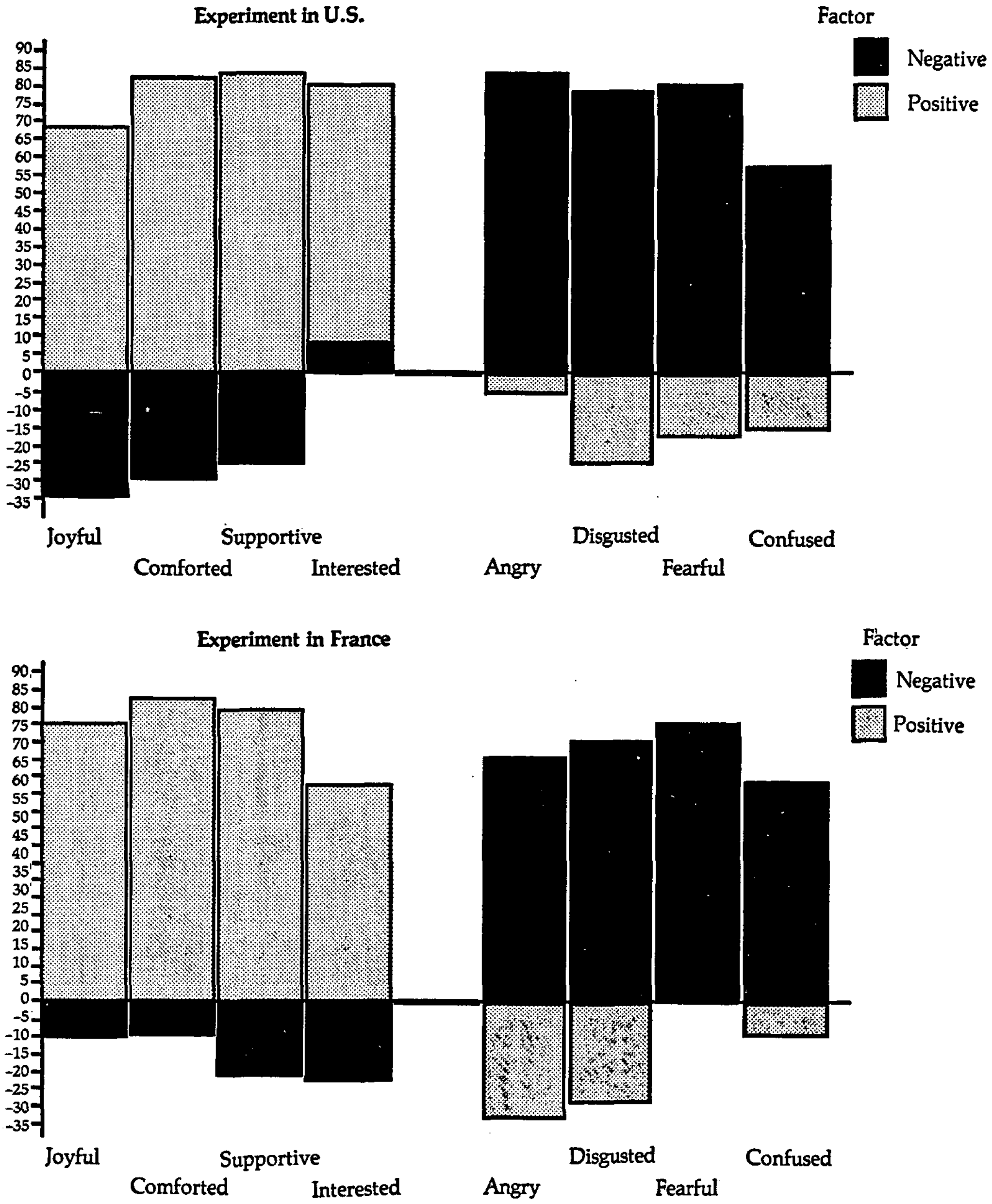
Such research shows that nonverbal behavior—traditionally viewed as a "natural" mode of communication (e.g., Rousseau 1964, 122–23)—indeed plays a central role in human politics, albeit in complex interaction with cognitive and culturally variable factors. In our experiments, videotape excerpts of known leaders exhibiting exemplars of facial displays of happiness-reassurance, anger-threat, and fear-evasion were selected on the basis of objective criteria derived from primate ethology and social psychology. When these stimuli were presented under controlled conditions, viewers' verbal—and in some cases psychophysiological—responses were recorded (for a review of methods, see Lanzetta et al. 1985).[2] Five findings are of primary importance for the demonstration that human behavior includes both hedonic ("positive" or pleasurable) and agonic ("negative" or competitive) dimensions, each of which has natural as well as cultural components.

First, viewers in both the United States and France discriminated accurately between displays, even though descriptive ratings were sometimes significantly influenced by political attitudes toward the leader being observed (Masters and Mouchon 1986; Masters and Sullivan 1989; Masters et al. 1986). Second, each type of display elicits a distinct pattern of emotional response at the psychophysiological level, indicating that verbal self-reports of emotion after viewing displays are congruent with "real" physical changes in the viewer (McHugo et al. 1985). Third, the verbal terms that viewers use to report these emotional responses reflect a similar structure in both France and the United States (see Figure 1), thus confirming the cross-cultural stability of the distinction between hedonic (positive) and agonic (negative) emotions previously reported in primate ethology, social psychology and political science (Chance 1976; Marcus 1988; Masters and Sullivan 1989). Fourth, at least for displays of happiness-reassurance across experiments using stimuli of many leaders, descriptions of increases in display intensity are highly correlated ($r^2 = .80$) with enhanced positive emotion (see Figure 2), indicating that this system of communication is unusually predictable once one controls for presentational style and viewers' attitudes (Carlotti and Masters 1989).[3] Finally—but most important for practical purposes—the emotional responses while watching leaders often interact with the viewers' prior attitudes or the political context in predictable ways, producing significant attitude changes of the sort that may influence contemporary elections (Carlotti and Masters 1989; Masters, 1989b; Sullivan and Masters 1988a, 1988b).

These studies thus show that the nonverbal cues of bonding (facial expressions defined as happiness-reassurance) have effects that differ from those of competition (displays of anger-threat and fear-evasion). Cultural ritualization does, however, shape the way these displays elicit viewer response. Hence, in France, excerpts showing a leader's anger-threat are as likely to elicit positive emotion in supporters as are that leader's display of happiness-reassurance. In contrast, partisans of a U.S. leader feel significantly less positive emotion after his anger-threat than his happiness-reassurance (Sullivan and Masters 1988b, 1989). While the distinction between social bonding and competition appears to be founded in a species-specific behavioral repertoire, this repertoire is in turn modified by cultural norms and traditions (Masters and Sullivan 1989).

The "three parts of the soul" described by Socrates in Plato's *Republic*, namely, desire, spirit, and reason (or the parallel Freudian principles of id, super-ego, and ego) thus correspond to distinct elements in a natural repertoire of social behavior. Contrary to the hedonistic tradition epitomized by Hobbes's psychology, humans are social animals for whom altruism or cooperation is a natural

Figure 1. Positive and Negative Emotions after Seeing Leaders



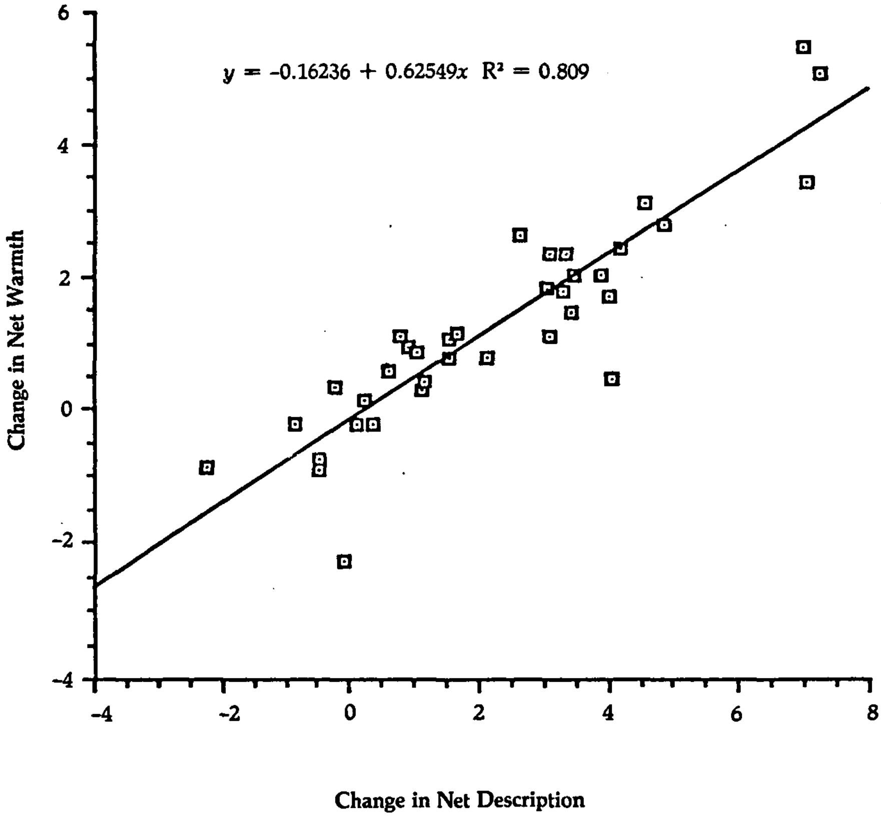
Note: Each bar represents factor loading on a scale for self-reported emotion; black bar = "negative" emotional factor; dotted bar = "positive" emotional factor. U.S. data from Experiment no. 2 (*n* = 145)—happiness-reassurance, anger-threat, and fear-evasion displays of President Reagan, all media conditions combined. French data from Nanterre experiment (*n* = 65)—happiness-reassurance, anger-threat, and fear-evasion displays of Prime Ministers Fabius and Chirac, all media conditions combined.

Source: Masters and Sullivan.

response. Indeed, the tendency to respond either altruistically or competitively in social situations seems to be a heritable trait, albeit one that interacts with individual experience (Hoffmann 1981; Rushton, Littlefield, and Lumsden 1986; Wispé 1978).

This does not mean that Hobbes and others adopting a hedonistic theory of human nature were simply foolish, however. Rather, pleasure-pain theories in psychology merely transpose, onto the individual, the cost-benefit calculus that evolutionary theorists now understand as the basis of natural selection. Utilitarians and behaviorists in the Hobbesian tradi-

Figure 2. Increased Description of Positive Cues and Enhanced Positive Emotion in Response to Videotape Excerpts of Leaders



Note: This figure shows the extent to which an individual leader's positive or happy display elicits stronger positive emotion than that same leader's neutral display. Each data point represents change in viewers' net warmth (self-reported emotional responses of happiness and comfort minus emotional responses of anger and fear) from the neutral to the most evocative positive display of a presidential candidate, compared with change in net description (descriptive ratings of strength and happiness minus descriptions of anger and fear) from the same neutral and evocative displays. Difference scores are based on means from comparable samples of college students and adults in experiments during the 1984 and 1988 campaigns (for method, see Sullivan and Masters 1988a). Correlations for individual scale scores range from .00 to .79.

Source: Carlotti and Masters 1989.

tion typically seem to assume that the individual is a tabula rasa or "black box," responding to stimuli in terms of pleasures and pains. In this view, universal "laws of behavior" reflect the contingencies of reinforcement of stimuli, without regard to the species and its evolutionary history.

While the experimental evidence confirms that the view epitomized by Hobbes does not describe the way individual organisms process information, something akin to it has become the basis of theories of evolution. For many biological theorists, social behavior can now be understood as the result of the differential transmission of genes that increase the "inclusive fitness" of the phenotype (Alexander 1979; Barash 1977; Wilson 1975). According to neo-Darwinian biology, natural selection—operating on populations over long periods—functions in a manner akin to the cost-benefit models of the utilitarian tradition. And this type of evolutionary theory is also central in any attempt to provide a naturalistic account of human politics.

Evolutionary Theory and the State

Despite the acrimonious debates concerning the presumed reductionism of "sociobiology" (Caplan 1978), the use of a cost-benefit calculus to explain human social and political behavior is characteristic of a long tradition in political philosophy represented by the social contract theories of Antiphon the Sophist and Thrasymachus in Plato's *Republic* as well as of Hobbes, Locke, and Rousseau (Masters 1982). Recently, both biologists and economists have shown that such a cost-benefit approach can explain how humans resemble other species in adopting varied behavioral strategies, depending on the net benefits to individuals (Alexander 1987; Frank 1988; Hirshleifer 1985; Margolis 1982; Maynard-Smith 1978).

From this perspective, social cooperation between close kin can be explained as a means of increasing individual "inclusive fitness." For similar reasons, it can be predicted that cooperation is more likely to occur between nonkin when they engage in repeated reciprocal interactions (Barash 1977; Trivers, in White 1981; Wilson 1975). Indirect reciprocity—the effect of an individual's reputation in a face-to-face group—can produce similar effects (Alexander 1987). In contrast, however, this theory predicts that mammals will *not* engage in indiscriminate acts of altruism or social cooperation that would benefit genetic competitors who are often both unrelated and unknown. Yet such benefits are precisely what is needed for the formation of the centralized state in which individuals die in defense of the community, pay taxes that benefit populations numbered in the millions, and otherwise engage in social behaviors of a scale not observed throughout almost all of hominid phylogeny (Campbell 1978).

To explore such issues, cost-benefit models derived from game theory are increasingly used to illustrate such questions in the behavior of other species as well as humans. For example, the so-called prisoner's dilemma has become especially fashionable as a model of the obstacles to cooperation among otherwise competing individuals (Axelrod 1983; Frank 1988; Maynard-Smith 1978). In this game-theoretic situation, even though a cooperative strategy would increase the joint benefits of the competitors, a rational calculus of cost and benefit leads each individual to a response that produces that greatest combined loss (see Table 1).

If the two competitors are close kin (e.g., siblings with a coefficient of relatedness of one-half), a prisoner's dilemma with the same payoffs becomes a cooperative game (see Table 2).[4] As Trivers and Axelrod have shown, repeated interactions—particularly in situations that en-

Table 1. Prisoner's Dilemma: Nonkin

	A's Strategy	
B's Strategy	Silent	Talk
Silent	A = 9 B = 9 mutual benefit	A = 10 B = −10 nepotism[a]
Talk	A = −10 B = 10 sociality or virtue[a]	A = −9 B = 9 mutual harm

Note: A two-person game is assumed in which communication or behavioral coercion between A and B is impossible and payoffs are transitive and commensurable (cheating > cooperating > joint punishment > virtue, or being a "sucker"). Payoffs = 10 > 9 > −9 > −10.

[a] Nepotism (private-interest; here, "cheating" by talking when other is silent) and sociality (virtue; here, being silent while the other talks) are defined in terms of A's payoffs.

tail role reversal—can make cooperation a dominant strategy among nonkin (Axelrod 1983; Trivers, in White 1981). And as Frank (1988) has more recently demonstrated, emotional responses like those described above can serve to make mutually beneficial cooperation accessible to competitors who would otherwise fall into the trap of responding to social situations merely in terms of short-term benefits. Hence, social strategies among humans are likely to be complex and situation-dependent, especially because individuals are likely to benefit from both deception of others and self-deception.

Since human nature is characterized by the tension between social and selfish impulses, much of the behavior we observe can be analyzed from an evolutionary perspective without being attributed to "genetic" causes totally beyond the realm of human endeavor. Because human behavior is so extraordinarily complex, however, conflicting pressures operate at the level of the individual, the group, and the species as a whole. Politics, in the conventional sense of the term, reflects the ubiquity and necessity of these conflicts between individuals, between groups within a single society, and between political communities in the international arena. Our species is, as Aristotle wisely said, the "political animal" (*zoon politikon).*

In suggesting that contemporary biology is relevant to human behavior as well as to the life of other animals, purpose and meaning are shown to be more—rather than less—important than might appear from conventional sociological or economic theory (Frank 1988; Masters 1989a). Scientifically observed regularities at the level of the entire species or the social system do not, therefore, entail rigidly determined constraints on the behavior of the individual. Conversely, the biological factors influencing human behavior at the individual level do not automatically produce social or cultural necessities at the political and legal level.

Table 2. Prisoner's Dilemma: Kin

	A's Strategy	
B's Strategy	Silent	Talk
Silent	A = 13.5 B = 13.5 mutual benefit	A = 5 B = −5 nepotism[a]
Talk	A = −5 B = 5 sociality or virtue[a]	A = −13.5 B = −13.5 mutual harm

Note: The same assumptions as in Table 1. Kin are either full siblings or a parent's offspring (i.e., coefficient of relatedness or $t = .5$). Hence, in each cell, A adds .5 of B's payoff in Table 1 to A's own payoff (and vice-versa).

[a] Nepotism (private self-interest) and sociality (virtue) defined in terms of A's payoffs.

The political institutions of the centralized state nonetheless pose a particularly difficult problem from the perspective of the life sciences. Even if humans sometimes cooperate (as do other animals), why did large-scale civilizations with governments replace the face-to-face groups found among other primates (Campbell 1978; Chagnon and Irons 1979; Gruter and Bohannan 1983)? Humans probably lived in small hunting-gathering bands for millennia and continued to form decentralized or stateless societies in many so-called primitive tribes well into the twentieth century. Politics can and does exist even where there is no centralized government (Masters 1964). How and why do states arise—and fall (Olson 1982)?

This question, long at the center of political philosophy, is also a critical issue in an evolutionary account of human life. Contemporary theories of natural selection explain the behavior and bodily structures of animals as "adaptations" that, all other things being equal, will be more successful in the replication of the individual's genes. Because most evolutionary theorists now argue that natural selection acts primarily on the individual rather than the group, social cooperation is usually expected to arise only when it results in a net long-term benefit to all partners (Alexander 1979; Barash 1977; Wilson 1975). Among mammals, as predicted by cost-benefit models like the prisoner's dilemma, cooperation is generally limited to close kin or to individuals who are likely to reciprocate. It is therefore hard to explain why humans have formed societies composed of millions of individuals and even harder to understand why some citizens are on occasion willing to sacrifice and die in defense of the centralized state.

How then did human governments arise? Since other mammalian species do not have complex social structures like those found in modern industrial communities, it is little wonder that social scientists have long assumed that biology can provide little insight into important problems of political behavior. When the "nature of the state" is considered from the perspective of contemporary evolutionary theory, the often puzzling status of political institutions can be clarified. In specifying the circumstances under which social cooperation might take the form of a centralized state or governmental system, moreover, this approach shows the linkages between historical studies of the origin of the state, political and social thought, and the life sciences. Indeed, the benefits and costs of a centralized government seem to be similar whether it is treated as a biological question, a problem in "rational choice" theories like those of economics and game theory, or an issue in traditional political philosophy.

This perspective provides the basis for an empirical analysis of the origins of the centralized or bureaucratic state, allowing a basic issue in political philosophy to be treated as a question for scientific analysis. Three principal hypotheses have been suggested for the origin of the state by political philosophers as well as contemporary anthropologists and biologists. For some, reflecting a tradition as old as Thucydides, the state arises due to the competition *between* social groups (Alexander 1979, 1987). For others, whose perspective resembles the thought of Plato or Marx, states are the reflection of broader forms of social cooperation *within* the group, arising when new technological, social, or political forms of life are discovered and implemented (Margolis 1982). Finally, a third approach—akin to the theories of Aristotle or Hegel—stresses the interaction between internal and external factors on the assumption that only a complex *feedback* process could have overcome the selective pressures against the formation of centralized states and bureaucracies (Corning 1986).

A feedback model not only provides the most plausible account for the emergence of the state but also suggests why individual leadership can be so important at critical moments in the history of centralized political regimes. Data from pristine states, such as the recently excavated site of Xochicalco in Mexico, are not entirely consistent with either the between-group or within-group models of state formation (Hirth 1984), suggesting that such a feedback process actually occurred. Indeed, without such models, it is hard to reconcile contemporary theories of state formation with the role of leaders in overcoming the obstacles to political success. Perhaps the veneration accorded to founders from Moses to Washington and Lenin reflects the objective need for human intelligence to overcome the many factors that can prevent the formation of the centralized states (see Machiavelli's *Prince*).

The emergence of civilizations and governments has not abolished the ambivalence of human nature at the level of the individual. The dialectic of cooperation and competition persists within the institutions of a state. Laws and customs become, in this sense, as much a part of the environment of human behavior as the topography and the weather. It follows that the fate of societies will often depend at least as much on psychology as on impersonal social forces. Since human individuals respond to their social context the way animals of other species respond to the ecological setting, political behavior cannot be understood without reference to the impact of the sociocultural and political environment on the individual's goals, feelings, and aspirations. What people think influences how they behave.

Consideration of how humans react within the centralized state also illuminates the functions and seemingly inevitable contradictions of bureaucratic organization (White and Losco 1986). Once in existence, the centralized state makes possible new forms of selfish exploitation of others as well as new dimensions of cooperation. Confronted with this environment, many individuals and groups tend to overexploit collective resources, ultimately leading to the collapse of centralized states (Olson 1982). As a result, an evolutionary perspective can explain how human nature is related to both the emergence of states and their repeated collapse throughout recorded history.

Although individual humans have a natural repertoire of behavior, social interaction takes on different forms in varied cultures and societies.[5] These modes of participation in human society can be explored by considering the circumstances in which individuals are excluded from participating in group activities (Gruter and Masters 1986). In addition to suggesting a way of classifying types of human society or regimes, this approach shows why the constitutional democracies of the West are closer to the species-typical range of human social behavior than are contemporary totalitarian or autocratic political systems. Biology and social science can indeed be effectively linked in the study of politics, pointing to the discovery of natural standards of justice.

The Implications of Naturalism in Political Theory

A return to naturalism on the basis of contemporary evolutionary biology challenges assumptions that have long dominated our thinking about human nature and politics. Contrary to the nihilism or relativism that have predominated in the West over the last century, an evolutionary approach to human social behavior offers a reasonable basis for judging the rightness or justice of political institutions. More specifically, the new naturalism provides objective criteria for preferring a constitutional regime in which citizens are subject to the law and play a legitimate role in political life. While evolutionary biology also explains the necessities that challenge civilization and its institutions, this naturalistic approach can help us to formulate more decent and humane standards of social life.

The study of human evolution points to principles that are neither as absolute as Kantian ethics nor as relative as contemporary nihilism. Although concepts of right and justice are not merely creations of human will with no foundation in nature, naturalism is contrary to rigid laws of nature that impose, on all situations, the perspective of any one time and place. What we know about human nature and the evolution of political or social institutions contradicts both moral relativism and dogmatic universalism.

Three broad principles—each broadly consistent with philosophical tradition—flow from a naturalistic perspective in political theory and ethics: (1) respect for individual and cultural differences; (2) virtue as a natural obligation to those benefiting from the advantages of civilized society; and (3) natural justice as a standard for ethical judgment. Each of these considerations can be based in evolutionary theory. None can give rise to the doctrinaire principles and totalitarian practices that have long been associated with attempts to claim biological superiority for a specific political or religious view of the world.

Respect for individual and cultural differences follows necessarily from the discovery of the natural causes of variation in human behavior. If the phenotype is merely the "vehicle" by which genes replicate themselves (Dawkins 1982), human beings are *equal* in a more profound sense than would appear from the conventional view of civil rights. Difference is not inferiority. Who can know which of us carries a mutant gene that is a valuable adaptation to a future environment and will someday spread throughout the human gene pool?

Nothing is more threatening to the survival of a species than the disappearance of naturally occurring genetic variation. Evolutionary principles teach us to expect humans to overvalue their own interests, perspectives, and importance. These principles also teach us, however, that this selfishness of the human phenotype is an evolved behavioral strategy that is naturally balanced by social cooperation, without which we could not have evolved or survived. Because human societies, particularly modern civilizations, depend on reciprocity, respect for others is enjoined on us by the very nature of the social system in which we live.

A theory of cultural evolution based on biological principles—like other theories in the social sciences—promises to account for social practices and laws as diverse as those of the !Kung bushmen and the ancient Indian caste system (Alexander 1987; Barash 1977; Wilson 1975). Unlike many other theories of human history, however, naturalism explains these differences in ways that suggest respect for human cultures different from our own. From an evolutionary perspective, we cannot pretend that our way of life represents progress over older, less sophisticated societies. Change is not always improvement.

Virtue is a rational foundation for ethical obligation within a civilized society. The form of social system we take for granted is extraordinarily rare in nature. Evolutionary principles lead us both to expect human selfishness and to recognize its dangers. To seek more for oneself may be natural, but historical circumstances have meant that for us, to live in a centralized state has *become* natural; and once the centralized state evolved, the virtues associated with this new social and cultural environment also emerged. Since each citizen benefits, both phenotypically and in reproductive success, from civilized social and political life, it is reasonable to expect that we will balance our own selfish needs not only by cooperation with others in the expectation of reciprocity but even by acts of self-sacrifice that contribute to the collective good.

The same principles that provide a foundation for the classical notion of virtue teach us to beware, however, of the use of this concept by the political leadership. The centralized state, as an institution, stands or falls according to the extent to which its citizens are capable of pursuing their own interests in a virtuous manner. But leaders as individuals are only too readily tempted to use this argument to their own selfish advantage. A sound ethical teaching can easily become a pernicious political ideology when the centralized state is used to enforce it as an orthodoxy since by so doing the first principle of naturalism—the respect for individual and cultural difference—is contravened.

Natural justice can thus be rediscovered as the foundation of our judgments of political and ethical manners. It follows from evolutionary principles that there is no single political system that can claim to be perfectly natural in all social and ecological conditions. Although this might seem an invitation to relativism, naturalism leads in a different direction. The question of what is—or is not—just cannot be entirely divorced from circumstances any more than can the question of what is—or is not—healthy food or exercise be defined abstractly for all humans in all environments (Aristotle *Politics* 4.1). A standard of justice can be both natural and changeable (Aristotle *Ethics* 5.7).

Just as the most evident measure of health is its absence, standards of natural justice are probably most useful in identifying *injustice*. Humans have adopted many different cultural and individual ways of life, none of which can claim to be ordained by natural selection in the sense that some religions ordain a mode of life as divinely given. But humans do have a natural repertoire of social behavior. When power or violence is used to exclude some from a civilized society, to silence them by precluding their participation in decision making and yet to prevent their exit, the basic element of reciprocity on which societies usually rest appears to be destroyed (Gruter and Masters 1986). Genocide and attempted genocide, using the power of the state as a means to destroy masses of humans, is naturally unjust.

As these conclusions suggest, modern biology illuminates the deep and perplexing issues that have confronted philosophers since the time of the Greeks (Fetzer 1985). More particularly, the problematic status of human nature in the Western tradition after Nietzsche (Deutsch and Soffer 1987) can be clarified by adopting an evolutionary perspective on human politics. Such an endeavor, which is as complex as any other natural science, leads toward a view of natural justice akin to that in Aristotelian thought. Since this perspective is profoundly different from the opinions generally attributed to Aristotle or from theologically based concepts of natural law, it is not vulnerable to the usual objections to a naturalistic analysis of human political and ethical choice.

A return to views like those of Aristotle on the basis of contemporary research in the life sciences will seem extremely paradoxical for several reasons. On the one hand, the textbooks tell us that Aristotle was apparently hostile to the concept of evolution. On the other, his political and biological theories were long used to support antiscientific orthodoxies. It is all the more difficult to keep an open mind toward the possibility of a return to naturalism because we have been told for so long that a scientific understanding of human nature needs to be value-free. Despite these habits of thought, it is essential to have an open mind when considering the implications of a scientific analysis of human nature, society, and politics.

The predicament in which we find ourselves at the end of the twentieth century is not trivial. Our species can obliterate itself with nuclear weapons, create genetic monsters, or destroy the physical environment through mindless pollution. Terrorists claim the intensity of their commitment is an adequate justification for their violence. Barbarians within and without challenge the concept of civilization as an unsubstantiated preference. Relativists scoff at the notion that there could be any reason whatever for standards of right and wrong.

Methodologists have long preached against the so-called naturalistic fallacy. It is indeed true that much sloppy thinking has used the term *nature* to legitimate prevailing beliefs and opinions. But it is also true that the supposed gulf between fact and value needs to be reconsidered in the light of what is now known about the nature of living beings. When the physician prescribes a treatment, we don't normally object that this practice is illegitimate because it bridges the logical distinction between the facts of diagnosis and the value of health. Biology, like medicine, has much to teach us about our species' nature and about the risks we confront by ignoring the natural basis and consequences of our habits of life.

A scientific understanding of human behavior should be presumed to be at least as difficult and demanding as nuclear physics, molecular genetics, or ecology. Unfortunately, it often seems to be assumed that because we are human, it will be easy for us to understand what humanity entails. Such an error is probably not unrelated to the events that have led our species to the brink of extinction. It is time to take seriously again the Socratic injunction, "Know thyself."

Notes

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1. This may appear an unlikely undertaking for a political theorist who studied under Leo Strauss, a philosopher who seemed highly critical of attempts to extend the scientific approach to the study of political things. Conventional opinions are, however, deceptive. Although Strauss and his students have argued that the task of scholarship is to recover the art of philosophic reading in order to understand the greatest thinkers of the Western tradition (Bloom 1987; Strauss 1953, 1964), Strauss himself was acutely aware of the importance of relating the study of human politics to the natural sciences. Indeed, he argued forcefully (1953, 7–8) that this was *the* issue of our times.

2. Each of these types of display has a dual name: the word before the hyphen represents a characteristic emotion, with the generally congruent social cue following the hyphen. Displays of happiness-reassurance reflect what psychologists usually call "positive" emotions or what ethologists describe as "hedonic" modes of behavior (Chance 1976, Ekman and Oster 1979, Lorenz 1970–71). Salient cues in these displays include a smooth, relaxed smile, tilting or movement of the head, raising of the eyebrows or visibility of the crowfeet at the eye, etc. "Negative" or competitive emotions and signals, called "agonic" by ethologists, contrast sharply and differ between anger-threat (brows lowered, eyes staring, head not moving, etc.) and fear-evasion (head lowered or turned away from target, eyes averted, lips tense or retracted, etc.). For a full description of these displays and the methods for selecting them, see Masters et al. 1986. For experimental studies, videotape excerpts showed clear exemplars of each type of facial display with minimal presence of hand movement or other distracting cues and were accompanied by a meaningful verbal message. In all studies summarized below, some viewers saw an excerpt with the sound and others with image only. Original verification of display effects also included presentation of displays with sound only (Masters et al. 1986; Masters and Sullivan 1989). While some interesting media effects have been noted—in general, for example, image-only presentations elicit stronger psychophysiological responses (McHugo et al. 1985)—responses are broadly congruent across communicative channels (e.g., Sullivan and Masters 1988a, 1988b). Results noted here therefore generally combine media conditions.

3. Direct measures of the effect of facial displays are complicated because viewers' responses also depend on the leader's individual style, the viewer's attitude toward the leader, partisanship, gender, etc., and the situation in which the excerpt is seen (Sullivan and Masters 1988a, 1988b; Masters and Sullivan 1989; Masters 1989b). To control for these interacting variables, the average descriptive rating of a happy-reassuring and a neutral display of presidential candidates in the 1984 and 1988 elections by an entire sample of subjects was used as a measure of display intensity. The difference between these two averages represents an assessment of the extent to which each leader's happiness-reassurance videotape excerpt exhibited greater expressive intensity than that same individual's neutral excerpt. In Figure 2, this difference score is correlated with the comparable difference in the "net warmth" (feelings of happiness and reassurance minus feelings of anger and fear) elicited by the same two displays for the same sample of subjects. These difference scores thus control for effects of merely seeing the individual leader's face as well as for between-subject variables that might influence emotional response, the nature of the experimental setting, the status of leaders at the time of the study, etc. While the individual scales for description of a trait (e.g., the candidate was "happy" or "angry") and the corresponding emotional responses (feelings of happiness or anger during the excerpt) only ranged from .0 to .79, the high-correlation coefficients of these difference scores (around .90) indicate that the evocative properties of the display do provide an additional element in viewer responses to leaders. Regression models demonstrate that these effects can influence posttest attitude (Sullivan and Masters 1988a, 1989). Examples of the practical consequences of episodic emotion include nationally televised political debates, such as those between Nixon and Kennedy in 1960, Chirac and Fabius in the French legislative elections of 1985, and Reagan and Mondale in 1984, and other events that may occasionally play an important role in electoral outcomes. The experiments described here thus seem to address Rousseau's challenge to modern philosophers (1964, 93).

4. It should be noted that the cooperation of siblings in a prisoner's dilemma depends on the precise ratio of payoffs to a greater extent than is the case for nonkin. Hence, the game matrix of Table 2 becomes competitive if the cost of "defection" is reduced from −9 to −3 (Masters 1989a, Fig. 5.4). A game with such a payoff for defection can, in turn, be converted back to a cooperative outcome if the punishment becomes more costly (Masters 1989a, Fig. 5.5). Because, as experience shows, the precise outcomes of social interaction depend on both the specific circumstances in the environment and the biological relationship of the participants, political theory needs to focus on the interactions between nature and nurture rather than on reductionist or single-cause models of explanation (cf. Aristotle *Politics* 1.1.1252a-6.1255b).

5. Lest the argument appear to be based on metaphor rather than scientific research, my forthcoming volume—*The Nature of Obligation*—will provide additional evidence that a naturalistic understanding of politics can inform the analysis of cultures and regimes. While empirical theories that explain variations in human social systems and norms challenge a widespread view that values are human creations in some sense freely chosen, such explanations of cultural and political differences are actually deeply rooted in our philosophical tradition (see, e.g., Marx and Engels' *German Ideology*).

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RESEARCH NOTES

VICTIMS OF CIRCUMSTANCES: WOMEN IN PURSUIT OF POLITICAL OFFICE

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Women have long faced special barriers in their efforts to gain election to political office. We show that the hurdles women encounter go beyond the often-described familial responsibilities and occupational disadvantages to include perceptual and political barriers unique to women. Using a two-wave, five-year panel of people serving on city councils, we find women likely to pursue higher office only under particular conditions—conditions that seem to matter little to men. Additionally, the success of women in pursuing higher office is more closely tied to the circumstances in which they find themselves than is the success of men. We suggest that the motivational circumstances of women and men in pursuing a political career are more complex than previously assumed. It is not just that men and women differ in their career attitudes and perceptions but that these attitudes and perceptions have different meaning for the two sexes.

Women seeking elected, public office are in a position unlike that of women in any other area of endeavor. Direct, public competition between men and women takes place only in electoral politics. Most sporting or athletic events separate the sexes, and there are separate awards in the entertainment industry as well. Electoral politics is about the only place where there is direct competition between men and women in a public forum. Elections require men and women to mount the same stage and the audience to compare their merits directly. Electoral competition not only produces winners and losers but shows an exact final score for all to witness. It is thus different from any other form of political or social activity.

If the situation of women in electoral politics is unique, the decision-making process for these women may be unique as well. The nuances of women's decisions to engage in this competition and the factors involved in women's stepping forward as political candidates have not been explored with sufficient care. Women are often assumed to make decisions about seeking political office in the same way men do, relying on the same information or perceptions and weighing them in the same fashion as men.[1]

Women operate under special circumstances in pursuing a political career. Women are less likely than men to obtain political office in the United States; and the higher the office, the less likely they are to obtain it. Most explanations for the underrepresentation of women emphasize conditions associated with women candidates and prospective candidates rather than with discriminatory voting among the public. The emerging body of research on women candidates and officeholders suggests that when women run for public office, they are not particularly disadvan-

AMERICAN POLITICAL SCIENCE REVIEW
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taged by their sex. The underrepresentation of women is due to a paucity of women candidates. Why are women not running for public office in larger numbers? The low incidence of women in elected public office is at least partly a response to very practical restrictions on political mobility imposed by child care responsibilities, a duty that is still disproportionately placed on women. Another explanation is that women are simply less ambitious than men. Several studies have documented lower levels of ambition among women (Diamond 1977; Kirkpatrick 1976; but see also Carroll 1985a), but the cause of women's lower ambition is less well explored. Political ambition is undoubtedly influenced by career choice and family obligations. It also may be a function of gender role socialization. Even well after the feminist movement has ostensibly shaken traditional gender stereotypes, ambition continues to be a characteristic associated with the male role (Ruble 1983). Alternatively, women may be as ambitious as men but decline to express that perspective because it conflicts with a desire to establish and preserve interpersonal relationships—a desire that is particularly strong in women (Eccles 1987). The plight of prospective women candidates and the conditions surrounding their political mobility comes out most clearly where women have been politically most successful—city councils.[2]

We investigate the career advancement of women in two ways. First, we examine the conditions surrounding the decision by women city councilors to *pursue* higher office by comparing those who pursue higher position with those who do not. We then focus on those who pursue higher office, comparing the successful to the unsuccessful. This will identify differences in the circumstances under which women are particularly advantaged or disadvantaged as political candidates.

This analysis of sex differences in upward mobility of city council members is based on information from local officials collected as a part of a two-wave, five-year panel study. Individuals first contacted in 1982 were recontacted in 1987 as part of a study of careers in city politics. Detailed sampling information is provided in the Appendix. We propose that the ability of individuals to move from the city council to higher office is a function of the political circumstances in which they find themselves. Specifically, these political circumstances include their political ambition, their perceived electoral security, whether they hold an at-large or a district council seat, and whether council elections in their city are legally partisan or nonpartisan. Our central inquiry concerns whether, within the context of these circumstances, sex differences exist in either pursuit of higher political office or in winning higher office.

The categorical nature of all variables in the analysis indicates the use of logit models. Those familiar with this technique recognize that even a large data set can be stretched very thin in testing multidimensional tables. A trade-off was made in the precision of several variables in order to include all variables in the model at once. Variables analyzed include

Ambition. Response to a question posed in 1982 regarding the importance the council member placed on obtaining a council seat as a stepping stone to higher political office. Those who cited this as a "very" or "somewhat" important reason for becoming a council member are compared to those who identified it as a "not very important" reason for seeking a council seat.

Margin. Response to a question posed in 1982 regarding the perceived closeness of the respondent's most recent council election. Those who viewed themselves as winning by a large margin and those who were unopposed are contrasted with those in close contests and those who won by a moderate margin.

Table 1. Competing Logit Models of Pursuit of Higher Office

Model	G^2	*df*	ChG^2	*Chdf*	Interpretation
1 [ABCDE][AF][BF][CF][DF][EF][AEF][BEF][CEF][DEF]	16.7	22	—	—	main effects and 2d-order sex effect
2 [ABCDE][AF][BF][CF][DF][EF]	25.4	26	8.7	4	main effects only
3 [ABCDE][AF][BF] [DF][EF][AEF][BEF] [DEF]	16.7	24	−8.7*	−2	no partisan effects
4 [ABCDE][AF][BF] [EF][AEF][BEF] [DEF]	17.2	25	.5	1	no direct ambition effect
5 [ABCDE][AF][BF] [EF][AEF] [DEF]	20.6	26	3.4[a]	1	no sex × margin effect
6 [ABCDE][AF][BF] [EF] [BEF] [DEF]	20.2	26	3.0	1	no sex × district effect
7 [ABCDE][AF][BF] [EF] [DEF]	23.4	27	6.2*	2	no sex × margin or sex × district effect
8 [ABCDE] [BF] [EF][AEF][BEF] [DEF]	26.8	26	9.6**	1	no direct district effect
9 [ABCDE][AF] [EF][AEF][BEF] [DEF]	25.9	26	8.7**	1	no direct margin effect
10 [ABCDE][AF][BF] [AEF][BEF] [DEF]	26.7	26	9.5**	1	no direct sex effect
11 [ABCDE][AF][BF] [EF][AEF][BEF]	27.3	26	10.1**	1	no sex × ambition effect

Note: Number of cases = 847. A = district vs. at-large council elections, B = margin of victory in last election, C = partisan vs. nonpartisan council elections, D = ambition for higher office, E = sex, and F = pursuit of higher office.

[a] For this and subsequent models, change is measured relative to model 4.

**Change in G^2 significant at alpha = .01.

*Change in G^2 significant at alpha = .05.

District. Whether the councilor's seat represented a geographic district within the city or was based on a citywide constituency. An at-large council position might reasonably be thought to offer a stronger platform from which to pursue higher office, as the elected official would have a citywide constituency to draw upon as a base of support. Those elected to at-large council seats are also more likely than district-elected councilors to have successfully appealed to a broad constituency in winning their position and therefore may have shown more potential for political advancement.

Partisan. Whether elections in the city are legally partisan—that is, with party labels on the ballots—or nonpartisan. Political parties are more active in legally partisan cities, and party organizations may be important vehicles of political recruitment. More importantly, legal partisanship and the accompanying strong party environment may provide a stronger linkage between local legislative and higher-level offices, enforcing a more up-through-the-ranks situation for political recruitment and restricting lateral entry into higher political office.

Sex. The sex of the politician.

Pursuit. Whether the individual pursued higher office within the five-year period.[3]

Success. Whether the individual was successful in winning higher office.

Modeling Pursuit of Higher Office

Table 1 presents the several models tested in fitting sex to pursuit in the context of political circumstances. The G^2, or likelihood ratio chi-squared, is a goodness-of-fit statistic based on the ratio of observed cell counts to counts that would be expected under a correct model. The "degrees of freedom" is equal to the number of cells minus the number of parameters in the model. The objective in model building, simply put, is to construct a parsimonious but powerful model by minimizing G^2 while maximizing the degrees of freedom. The impact of inclusion or exclusion of parameters or combinations of parameters can be tested by referencing a chi-squared table.[4]

We centered our model building around sex, exploring for first- and second-order sex effects but not probing for second-and-higher-order effects of other variables nor examining third-and-higher-order sex effects. The first model of main effects and second-order sex effects thus provide a sort of "target" G^2 with which to compare subsequent models as we attempt to approach the explanatory power of this model with one containing fewer parameters.

Comparing model 1 with a main effects model, model 2, reveals the total impact of second-order sex effects beyond the direct effects of all variables. The 8.7 increase in G^2 indicates that sex-interactive effects *do* contribute to the model. However, at four degrees of freedom the combined contribution of all four second-order effects is statistically insignificant.

In a first revision of the two models, all partisan effects are removed. The resulting model 3 matches the fit of the first model with two fewer parameters. Partisan/nonpartisan local elections offer no explanation of the likelihood of pursuing higher office, nor does sex work within this context in influencing pursuit. The ease with which the sex × partisan elections effect is removed from the model suggests that political parties play a minimal role in the *recruitment* of women from the council to higher office. The model is further improved by eliminating the direct effects of ambition (model 4), a deletion that increases the G^2 by only .5, easily insignificant at one degree of freedom. Within the context of this model, ambition has no direct effect on the likelihood of pursuing higher office.

We next turn our attention to the effects of district versus at-large elections and marginality. In removing sex × marginality (model 5), we notice a distinct deterioration in the model, but one that is not statistically significant at the .05 level.[5] Similarly, in removing sex × district elections (model 6), a noticeable but insignificant change in G^2 is seen. However, when in model 7 both variables are omitted, the 6.2 reduction in G^2 is statistically significant at the .05 level. Thus, both parameters are left in the model. Sex has an impact on pursuit by way of both marginality and district versus at-large election. Further, as models 8 and 9 reveal, the direct effects of these two variables play important roles in determining the likelihood of local legislators' pursuing higher office as well.

In model 10 the direct effect of sex is removed, while the interactive effects of sex and marginality, sex and district versus at-large elections, and sex and ambition are left in the model. The omission of sex causes an increase in the G^2 comparable to that observed with the elimination of district elections, a change that is statistically significant at .01. Clearly, within the context of the model, sex has an important impact on the likelihood of a council member's pursuing higher office.

Finally, the interactive effects of sex and ambition are removed in model 11. Recall that the *direct* effects of ambition easily came out in model 4. Here we observe the real power of ambition. Sex × ambition has an adverse impact on G^2 of 10.1—greater than any other parameter in the model. Political ambition has an impact on the likelihood of pursuing higher office that can *only* be understood in the

Table 2. Probabilities for Pursuit of Higher Office According to Sex, Marginality, Ambition, and Election Type

	Male		Female	
	Ambitious	Unambitious	Ambitious	Unambitious
District				
Safe margin	.25	.15	.07	.13
Unsafe margin	.31	.12	.02	.04
At-large				
Safe margin	.32	.19	.19	.31
Unsafe margin	.26	.15	.06	.12
Number of cases	651		196	

context of the sex of the individual politician.

Rejecting models 5–11, we conclude that model 4 offers the most accurate yet parsimonious description of the role of sex on pursuit of higher office in the context of these political circumstances. The likelihood of pursuing higher office is directly linked to the individual politician's retrospective assessment of electoral security, whether the politician was elected to the city council from an at-large seat or a district seat, and the sex of the individual. Additionally, marginality, district versus at-large election, and particularly political ambition affect pursuit differentially according to sex.

Table 2 shows the proportionalities of individuals who pursue higher office under the conditions established in model 4. Here we can observe directly and vividly the circumstances under which women and men attempt to gain higher office.

Politicians who regard themselves as electorally secure are more likely to make a bid for higher office than those who see themselves as vulnerable. However, women are much more likely to be influenced by this perception than men. Secure women are two to three times more likely to seek higher office than insecure women, regardless of election structure and ambition level. Among men, the difference is far less, usually only a few percentage points. Similarly, both males and females are more likely to pursue higher office from the vantage point of an at-large council seat than from a district council seat; but again, the difference is particularly noticeable among women. Women elected at large are two to three times more likely to seek higher office than district-elected women, far exceeding the differences observed among men.

Finally, and most strikingly, is the impact of political ambition. When pursuit of higher office is examined in the context of men's and women's ambition, we see totally different processes at work. Ambitious men, as might be expected, are much more likely to seek higher office than men who lack ambition. Indeed, political ambition seems to be the only trait that has a substantial impact on men. Ambitious women, on the other hand, are actually less likely to seek higher office than those without ambition, a condition that persists under all conditions of marginality and election structure. The point is not only that men are more ambitious than women (our data show males twice as likely to acknowledge ambition for higher office as females), but that ambition has direct and significant meaning in determining the behavior of men, but not women.

There are several possible explanations

for these findings. It may be that women need the stronger position or vantage point offered by an at-large council seat to feel capable of launching a bid for higher office. Alternatively, the process of *obtaining* an at-large seat may weigh more heavily upon women than men. Women who obtain an at-large seat position are more upwardly mobile relative to district-elected women than are at-large elected men relative to district-elected men. Assessing the meaning of the roles of ambition and marginality is more problematic. The major issue is one of the depth of the relationships identified in Table 1 and described in Table 2. For both marginality and ambition, the findings can be interpreted as due either to sex-based differences in response tendencies or to more fundamental differences in the roles of marginality and ambition in career decision making. Women may be socialized to disavow ambition because ambitious women are seen as "pushy" even though this trait may be admired in males. Hence, women deny that pursuit of elected office is to further their careers and claim they desire only to serve the community. Women giving the most serious thought to making a bid for higher office may feel the most pressure to disavow political ambition.

Sex-based differences in response to an ambition question may be more than simply a case of differing response tendencies and may reflect real differences in motivation. To the extent that women accept and internalize gender roles that describe the female as communal, nurturant, and other-regarding, they may in fact be more likely to pursue higher office out of a sense of service to the community rather than out of a desire to enhance their own careers. This is consistent with findings that women are less likely to perceive their involvement in politics in "professional" terms of competitive strategies than in terms of "amateur" commitment to a principle (Lynn and Flora 1977).

Alternatively, sex differences in the role of political ambition may resemble what psychologists have found in linking the "achievement motive" (Atkinson 1958; McClellan 1961) to subsequent performance of men and women. The achievement motive has been found to offer a good indication of subsequent performance of male subjects but a poor and largely valueless indicator of the subsequent behavior of females. Most explanations for these sex differences are rooted in the idea that women often operate under competing motivations that serve to negate or diminish the achievement motivation—performance linkage. Women score much higher on an "affiliative motive" involving a concern for family, friendships, and interpersonal relations. High levels of affiliative concern make women's motivations more complex. Whereas men tend to devote single-minded attention to achievement and performance, women combine achievement needs with concerns about the effect of achievement on family and personal relationships.[6] Role conflicts and competing motivations serve not only to diminish the political ambition of women (Sapiro 1982) but also to diminish the importance of ambition as an indicator of future career behavior.

The interpretation of our findings for marginality also admits several possibilities. In reporting their margin of victory in the previous election, women may have tended to be more modest, and more accurate in their evaluations of their situation. Research has shown that men are more likely to exaggerate their success and make self-serving attributions about their accomplishments (Crandall 1969), while women rely more on external indicators of success (Meese and Watts 1980). If true, women's more accurate assessments of electoral security may explain why this measure of marginality more sharply identifies upwardly mobility among women compared to men. Alter-

Table 3. Competing Logit Models of Success in Winning Higher Office

Model	G^2	*df*	*ChG*^{2}	*Chdf*	Interpretation
1 [ABCDE][AF][BF][CF][DF][EF][AEF][BEF][CEF][DEF]	24.8	16	—	—	main effects and 2d-order sex effects
2 [ABCDE][AF][BF][CF][DF][EF]	29.3	20	4.5	4	main effects only
3 [ABCDE] [CF][DF][EF] [CEF][DEF]	25.6	20	-3.7	—	no effects for district or margin
4 [ABCDE] [CF][DF][EF]	30.0	22	4.4	2	no sex × ambition or sex × partisan effect
5 [ABCDE] [CF] [EF] [CEF]	27.5	22	-2.5	—	no ambition effect but with sex × participation effect
6 [ABCDE] [CF] [CEF]	27.5	23	.4	1	no direct sex effect
7 [ABCDE] [CF]	32.6	24	5.1*	1	no sex × partisan
8 [ABCDE] [CEF]	40.4	24	7.8**	—	no partisan effect but with sex × partisan effect

Note: Number of cases = 163. A = district vs. at-large council elections, B = margin of victory in last election, C = partisan vs. nonpartisan elections, D = ambition for higher office, E = sex, and F = success in winning higher office.

**Change in G^2 significant at alpha = .01.

*Change in G^2 significant at alpha = .05.

natively, if women are accustomed to waiting for invitations to participate "in areas of human behavior ranging from courtship to job promotions," it is conceivable that they wait on invitations to run for political office as well (Carroll 1985b). Electorally marginal women may simply never be asked. Marginal men may not be asked either, but men are less dependent upon invitations. They run whether asked or not.

Modeling Success

Of course, candidacy for political office is one step removed from the prize itself. The number of women seeking political advancement and the conditions influencing their efforts would be irrelevant if the candidacies of women were doomed to failure. Having identified the conditions associated with pursuit of higher office, we now direct our attention to the political conditions linked to *success* in pursuing higher office. For this analysis, the data set is reduced to individuals who ran for higher office.

Table 3 shows the process by which success was modeled using the same conditions as before. The main effects and second-order sex effects model yields a G^2 of 24.8, 4.5 less than the model with main effects alone. This immediately suggests that second-order sex effects play a smaller role in determining success.

The model is pared by removing direct and sex-interactive effects of district versus at-large election and marginality (model 3), yielding a G^2 within 1.0 of the initial model but with four-fewer parameters. Removal of sex × ambition and sex × partisan elections (model 4) produces an increase in the likelihood ratio chi-squared that is noticeable but not statistically significant. A better model was achieved by restoring sex × partisan elections but omitting the direct effect of ambition (model 5).

Model 6 deletes the direct effect of sex with almost no deleterious impact on the G^2. Thus, everything else being equal, women are as likely as men to win higher office *when they make the attempt*. Further refinements of this model were attempted: first, by again removing the interaction of sex and legally partisan council elections (model 7), which produced a statistically significant increase in the likelihood ratio chi-squared ($p = .05$); then, by restoring sex × partisan elections and removing the direct effects of partisan-nonpartisan election format (model 8). The latter modification causes a major reduction in the accuracy of the model (change in $G^2 = 7.8$), clearly demonstrating the importance of this variable in determining the likelihood of local legislators' successfully obtaining higher office. Model 6 clearly offers the most concise explanation of success in pursuing higher office.

The linkages between partisan versus nonpartisan elections, sex, and success in pursuing higher office are shown in Table 4. Both men and women are more likely to be successful in moving from city councils to higher elected position when council elections are legally partisan, though the effect is particularly acute for women. Thirty-four percent of men who seek higher office from a partisan council can expect to win, compared to about 20% who seek higher office from a nonpartisan council. In contrast, women running from a partisan council have an expected success rate nine times greater than that of women running from a nonpartisan council.

The importance of this effect in the success model suggests a significant reinterpretation of the role of parties in the recruitment of women to office. Our data show that partisan elections for council are unrelated to pursuit of higher office but have an impact on success in achieving higher office. Strictly speaking, political parties do not "recruit" women candidates to higher office. Parties provide a vital linkage mechanism for women and other traditional outsiders who do not have the business and social connections often associated with a successful campaign, personal connections that men are more likely to possess. However, if by "recruitment" one refers to the encouragement of candidacies early in the campaign process, we see no sign that women are advantaged in strong party environments. Any evidence gathered from successful women officeholders regarding the importance of parties in their recruitment may thus be misleading and may tend to give parties more credit than they deserve for furthering the representation of women in elected office.

Table 4. Probabilities for Success in Winning Higher Office According to Election Type and Sex

	Male	Female
Partisan	.34	.55
Nonpartisan	.21	.06
Number of Cases	151	35

Victims of Circumstances

Perhaps the clearest conclusion to emerge from this study is the riskiness of drawing direct comparisons between men and women on the basis of attitudinal research. When we ask men and women about their political ambitions we obtain information that means one thing for men, another for women. Even working with individuals elected to local office, whose selections should minimize sex-based differences in perceptions, we see major limits on our ability to link attitude to behavior in a straightforward manner. Presumably, in samples of individuals less sifted through by political processes—

party activists, for example—sex differences would be even greater, while differences among higher levels elites should be smaller. Future sex or gender research must take into account the problematic nature of linking attitudes to behavior of men and women.

The barriers women face in achieving anything approaching proportionate political representation are certainly more numerous, and probably more onerous, than previously recognized. Compared to men, women are more likely to be influenced in making a bid for higher office by the strength of their current political position and their perception of their political vulnerability. Women who see themselves as electorally vulnerable are unlikely to try for higher office.

The circumstances in which male local officeholders find themselves have little impact on their decisions to pursue higher office or their success in pursuing higher office. Men are more likely to be self-motivated—guided by political ambition—and appear only modestly affected by these other circumstances. Referring back to Table 2, note that within the categories of political ambition there is little variation among men—only about six or seven percentage points—but great variation among women in the likelihood of seeking higher office. If men are politically ambitious, these other considerations largely do not matter. They are of great importance to women.

Two factors may be at work in liberating men from the burdens women face in pursuing higher office. First, men are more likely to have the social and business connections or networks that facilitate their candidacies. Thus, males are less likely to need an at-large council seat as a platform for higher office. Males are less in need of a political party to win higher office because they have these other resources.

But beyond this condition there may exist a psychological impediment to the political advancement of women. Men pursue goals of whatever type with a singlemindedness absent in women. In the competitive world of electoral politics singlemindedness can be a vital asset, particularly in reaching for higher-level positions. Society faces a dilemma in adapting political processes to accommodate two distinctly different perspectives of women and men. Some changes in political structure and process would clearly benefit women candidacies. For example, strengthened political parties and the expansion of at-large seats would increase political opportunities for women. However, these reforms will not address motivational differences between the sexes. The current system of obtaining political office—especially posts higher than local office—is highly individualized and competitive, emphasizing characteristics that are fundamentally inconsistent with women's status and role orientations. Willingness to engage in this political competition requires a driving ambition for personal advancement that disregards or discounts other social responsibilities and personal relationships. Women, with their more balanced system of values and priorities, are at a distinct disadvantage in this competition.

Appendix

City council members serving in cities between 50 thousand and one million population were contacted via mail questionnaires in the spring of 1982 and again in the spring of 1987. The 1982 wave achieved a response rate of 61% with a total of 975 participants. Of these same officials, 805 were successfully recontacted five years later. This represents 85% of the original participants less 22 deceased individuals and 9 for whom no personal identification was possible. The 85% response rate reflects the results of a four-wave contact schedule, culminating in a certified letter to nonrespondents. Second-wave questionnaires were validated by comparing birth years solicited in both studies. Inconsistent reportings that could not otherwise be explained caused the questionnaire to be discarded.

Several potential biases in the sample were investigated. First, a concern that mail survey respondents differ from nonrespondents in career behavior lead to a comparison of the likelihood of the two groups' continuing in office over a five-year period. Information on the membership of all councils obtained in a separate survey of city clerks indicated no significant difference between respondents and nonrespondents in the general measure of career behavior.

The panel is somewhat overrepresentative of continuing council members. Although officials remaining in office through the five-year period comprise just over 45% of council members in the population, those continuing on the council represent 52% of the panel. Basic career information for an additional 114 individuals was obtained in a separate survey of city clerks. For the analyses we report, this basic information about career path was sufficient to allow for the inclusion of these cases. The addition of these 114 cases brings the total sample to 919, or about 95% of the original 1982 sample and eliminates the bias in response rates.

Information obtained in the original study permitted a thorough comparison of panel participants to nonparticipants. Of the several dimensions examined, only sex was found to differ between panel members and nonparticipants, women being somewhat overrepresented in the panel. Most of this difference can be attributed to sex differences in mortality rates.

Aside from behavioral and situational variables, two perceptual indicators are used. In regard to their ambition for higher office and their perceived marginality in their most recent council election, respondents were asked, "We would like to know why you initially decided to seek election to the city council. Please rate the importance of . . . [As a stepping-stone to some other political office] . . . in influencing your decision to run for a council seat" *(very important, somewhat important,* or *not very important);* and "How would you describe your margin of victory in your last campaign?" *(I was unopposed, I won by a moderate margin, I won by a large margin,* or *it was in a close contest).*

Notes

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1. An exception is Sapiro (1982), who argues that women and men differ in how they calculate the costs and benefits of running for political office.

2. City councils and other local offices are not generally regarded as important rungs on political career ladders in the United States (Schlesinger 1966), but their importance may be greater for the politically disadvantaged than for others. Whites, males, and the wealthy may be able to use their status as business leaders, labor leaders, and political insiders to move directly into higher political office without establishing themselves in lower offices. Those lacking these advantages—minorities, women, and the less wealthy—need manageable first or second rungs on the career ladder, such as may be afforded by a seat on the city council. Thus, city councils offer a lower-stakes political entry point for those not yet ready for high-stakes politics.

3. Any office that would normally be considered a move up from the city council was included. Thus, citywide executive office, county office, state legislative office, statewide office, and congressional office were all part of this opportunity structure.

4. See Freeman 1987 for a more detailed discussion of logit model building and interpretation.

5. For models 5–11, the change in G^2 is measured relative to model 4.

6. A parallel expression of this situation is found in Hacker's (1951) concept of "marginality," the simultaneous maintenance of two roles for women who enter male-dominated pursuits and the inevitable conflict between those roles. See also Githens and Prestage 1977 and Sapiro 1983.

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THE ELECTORAL RELEVANCE OF LOCAL PARTY ORGANIZATIONS

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We examine the role that one group of party units—county party organizations—play in electoral politics, based on electoral and county party organizational data collected during 1980–84. Local party chairs report their organizations are involved in a number of electorally relevant activities, including candidate recruitment, joint planning with candidate organizations, and various independent campaign activities. The data demonstrate that county party organizations are indeed effective. The probability of a minority party's running candidates for lower-level offices, which appears to contribute to higher vote totals for higher-level offices, is a function of the local strength and activity level of the party; whereas direct effects are seen to be small. These data suggest that even if mainly at the candidate recruitment stage of the process, party organizations play an important role in local electoral politics.

The weakening of partisan attachments in the U.S. public is among the most extensively documented phenomena in U.S. politics (see, e.g., Norpoth and Rusk 1982, Wattenberg 1984). The attitudinal and behavioral manifestations of this trend include increasing numbers of independents, a rise in split ticket voting, and a lessening of the relative importance of partisan identification for individual voting decisions (Nie, Verba, and Petrocik 1976). It has even been argued that this decline of the "party-in-the-electorate" is part of a general decline in U.S. political parties. The conclusion expressed by Maisel (1981) is typical: "For the parties to have control they must be viable as organizations; if candidates can continue to run their own campaigns, stressing personality and not issues, ignoring the party regulars for the appeal of the media, then *party as an organization will continue to atrophy* and parties will continue on the road to electoral irrelevance" (p. 44–45, emphasis added).

At the same time, a detailed examination of party organizations by Cotter and his colleagues (1984) has documented the health and widespread influence of national, state, and local party organizations during the late 1970s (see also Crotty 1971). An extension of their analysis of local party organizations through 1984 shows continued strength and vigor (Gibson, Frendreis, and Vertz 1989). Taken together, these studies present a seeming paradox, pitting evidence for continual—and perhaps increasing—organizational vitality of political parties against a growing uncertainty over the relevance of parties within the electoral context.

In this note we address this issue of the electoral relevance of contemporary U.S. political parties. The central question of concern is simply, "Do party organizations matter?" Our focus is on one level of party organizations, county-level organizations. Our analysis proceeds in two parts. First, we discuss the position of local party organizations within the electoral process, including a brief review of existing work concerning elections and party activity. Second, we examine the relationship between local party organizational activity and electoral outcomes for the 1980, 1982, and 1984 elections.

Political Parties and Elections

Not all evidence on party system change supports a description of a declining electoral relevance for political parties. In terms of winning offices, the electoral domination of the Democratic and Republican parties is at a historically high level (Schlesinger 1984). To be sure, recent elections have been taking place against a backdrop of growing partisan independence—or perhaps neutrality (Wattenberg 1984)—in the mass public. Even this, however, is not unambiguous evidence of party decline. If, following Schlesinger (1984) and Downs (1957), a party is conceived as a team seeking to control offices by winning elections—as distinct from those in the public who vote for the party's members—the decline in mass partisanship is more properly seen as a rise in the uncertainty in the partisan environment rather than a decline of the organizations themselves.

A number of studies have directly examined the connection between party activity and electoral outcomes.[1] In case studies of large cities during the 1950s, Katz and Eldersveld (1961) and Cutright and Rossi (1958) found positive correlations between precinct-level party activity and vote totals. Crotty's (1971) analysis of North Carolina counties also detected a moderate connection, although Pomper, Moakley, and Forth (1980) found little relationship between party activity and electoral outcomes in New Jersey. In one of the few multistate studies, Patterson and Caldeira (1984) observed a strong effect of party organizational activity on interparty competition, although the effects on electoral outcomes—who wins—was not examined (see also Barrilleaux 1986). *State* party organizational strength has been shown to have a significant impact on gubernatorial elections (Cotter et al. 1984), but the recent evidence concerning local (e.g., county-level) party activity is less clear. Gibson and Smith (1984) found some statistical relationships between party activity and gubernatorial voting outcomes, but the relationships showed tremendous variability across states and parties with respect to both magnitude and direction. Finally, Herrnson (1986) found that congressional candidates regarded party organizations at all levels to be important sources of assistance for their campaigns, in contrast to the view that candidate-centered organizations must necessarily compete with regular party organizations.

Although suggestive, the limited geographic scope or earlier time frame of much of this research leaves open the question of the contemporary electoral relevance of party organizations.[2] In addition, most existing analysis has proceeded without a clear understanding of the strategic goals and opportunities parties identify for themselves. In the U.S. party system of loosely coupled organizations, local organizations must first be thought of as independent actors, determining which activities they will undertake to achieve goals they themselves define as attainable. Without an understanding of the avenues of influence that parties define for themselves, conclusions about the efficacy of party organizations are misplaced.

As a starting point to analyzing party organizations as independent electoral actors, political parties should be seen as strategic actors operating within particular environments. Like Schlesinger (1984), we assume the goals of political parties are to contest (and ultimately win) elections. In this view, the ultimate electoral objective of a party is not to maximize the number of people who express an attitudinal preference for it but, rather, to be able to contest elections effectively. Depending on the existing partisan context, this long-range objective may be pursued by different tactical approaches. For example, in areas of one-party dominance the minority party may follow a long-term developmental approach that includes establishing a token presence, developing a cadre of local activists, and gradually recruiting (or converting) credible candidates. Although the level of mass partisanship may help define the context of this partisan activity and structure the strategic thinking of party leaders, the electoral process, not mass attitudes, is the object of partisan planning and activity. In addition, specific behaviors of party organizations will result from an interplay between the resources of the organization (physical and human capital) and the constraints and opportunities created by the organizational environment (see, e.g., Beck 1974). For example, the attention given to various races is likely to be a function of the majority-minority status of the party locally, statewide, and nationally. An organization in an early stage of the party-building process may focus its efforts on candidate recruitment rather than on mass efforts like voter registration.

Electoral Role of County Party Organizations

Our examination of the electoral roles of local political parties is for county-level organizations in the United States in 1980 and 1984. The Party Transformation Study (PTS) surveyed the universe of local (county and county-equivalent) party organizations in the United States in 1979–80, with a resulting response rate of 53% (for details, see Cotter et al. 1984). In 1984, we resurveyed a stratified probability sample of 1,000 of the PTS-responding organizations. The resulting observations for 1980 and 1984 from 626 county party organizations thus constitute a panel design. (A copy of the questionnaire and further details on research design are available from us.) Where we wish to make comparisons between Democratic and Republican organizations, the analysis reported is based on the 442 organizations in the 221 counties in which both the Democratic and Republican chairs responded in 1984.[3]

Elsewhere, we have presented a detailed discussion (based on these data) of the organizational strength and activities of local parties, including an analysis of the relationship between local party organizations and their organizational environment (Gibson, Frendreis, and Vertz 1989). Three findings drawn from this analysis are most relevant to our current concern. First, the local organizations engage in a substantial amount of direct campaign activities (e.g., voter registration drives or the distribution of voter guides). Far from withering away, the trend between 1980 and 1984 is in the direction of greater organizational activity. Second, in addition to these direct campaign activities, virtually all of the county party organizations are also involved in the campaigns of individual candidates, particularly those running for lower-level offices. The involvement of the county organization begins with candidate recruitment, extends through the primary, and continues during the general election. Third, the county chairs see their organizations as significant electoral actors. Most chairs perceive their organizations' activities as effective with respect to the electoral fortunes of candidates at all levels, although in both parties the chairs see their organizations as being most effective in races for local offices (county and state legislative). What this earlier analysis did *not* demonstrate is that these party organizations *actually* influence politics.

Party Organizational Activity and Electoral Outcomes

Like most organizations, some of the events local parties wish to influence are more directly accessible to the parties than are others. Operating within the world of the more politically active sector, party organizations are most directly connected to other organizations and individuals within this milieu and less directly connected with the mass of voters. More concretely, this means that party organizations should have a greater *direct* impact in areas like recruiting and coordinating elite support for candidates than in the mass persuasion of voters. In contrast, candidate-centered organizations exist precisely to mobilize and convert voters to a candidate's cause, and these organizations deploy their resources accordingly. Party organizations, with their longer-range and multicandidate perspectives, are better situated, and more motivated, to concentrate on the less visible, institution-building aspects of the electoral process. Even direct campaign activities undertaken by local organizations (e.g., voter guides and rallies) can be viewed (in part) as part of the "package" of support used by some organizations to attract reluctant candidates for difficult races, rather than as efforts at mass persuasion of voters. Thus, party organizations need not compete with candidate campaigns. Instead, they provide the sort of institutional support and infrastructure that few campaigns are motivated to develop.

We therefore expect that the campaign activities undertaken by local parties are likely to have only a marginal direct effect on overall vote totals. Several decades of voting research attests to the dominant influence of such factors as the attitudes of individual voters and the actions of the candidates and their organizations. Within this context, the electoral activities undertaken by local party organizations are likely to operate only to reinforce these other factors, for example, increasing the name-recognition of party candidates or mobilizing marginal voters. We therefore expect to see only modest direct effects between local party activities and voting outcomes.

In order to assess the impact of party activities on votes, we have examined the relationship between several party organizational variables and county-level electoral returns for six offices: president (1980 and 1984), governor (1982), U.S. senator (1980, 1982, 1984), U.S. representative (1980, 1982, 1984), and the upper and lower houses of the state legislature (1984).[4] In addition to the overall level of campaign activity of each county organization, we have included in the analysis summary scales measuring the structural strengths of the party organizations.[5] We assume that structural strength in part reflects the capacity of the organization to undertake a variety of electorally relevant activities, including—but not limited to—the general campaign activities comprising the activity scale. Its inclusion in the analysis allows us to detect less immediate effects between party organization and electoral outcomes. The electoral outcome variables are the Republican percentages of the two-party vote for each race.

To assess the independent effect of local party structural strength and activity, we regressed the aggregate voting results on the party organization variables. Separate equations were estimated for each office for each year. To control for the underlying partisan predisposition of the county, we first entered four demographic variables (all from 1980) into the equations: population rank of the county, percentage of adult population finishing high school, percentage rural farm population, and percentage below the poverty line. As is evident in Table 1, the effects of county-level party structural strength and campaign activity on county-level voting outcomes are weak. On average, the county party variables add between two and three percentage points to the explained variance, after the demographic variables are taken into account. This is quite small, although it is not a trivial portion of the total variance that can be explained.[6]

Why are local party strength and activity so weakly related to election results? One problem may be the use of linear modeling techniques on a large cross-section to address what may be (for some organizations) a longitudinal or nonlinear process. In some cases the strongest electoral payoffs for local party activity may come under fairly limited conditions, for example, when a moribund organization is revived, when a long-time incumbent retires, or immediately after redistricting.[7] An intensive qualitative analysis of a few organizations may be required to uncover these complex, conditional relationships.

More importantly, though, the mass persuasion of voters is just one of the ways in which local party organizations might seek to affect the electoral process. As we have noted, organizational theory would suggest that these organizations are more likely to be able to affect elements of the population that are most directly accessible to the organization—in this case, other members of the local political elite rather than the mass population. If this is the case, one obvious manifestation of this process should be that stronger and more active local party organizations should be more likely to be able to recruit candidates for local races and hence contest a greater number of offices. Though less obvious than an election day get-out-the-vote drive, effective candidate recruitment is an important element in contesting and winning elections (see, e.g., Jacobson 1987), both in an immediate sense and in the long run.

To test this possibility, we have reanalyzed our data with regard to a different electoral outcome, focusing not on the percentage of the vote received but on whether or not a party ran candidates for office. In the mid 1980s, there remain many places, particularly in the South, where candidates for lower-level offices (and, less commonly, the U.S. House) continue to run unopposed. The inability to field candidates is an obvious impediment to winning elections.

The particular question of interest is whether the strength and activity levels of the county Republican party are associated with greater probabilities of Republican candidates for the lower house of the state legislature and the U.S. House appearing on the county ballot. We have focused on the activity of the Republicans because they generally represent the less electorally active, minority party within the time frame covered by our data. As a consequence, there is much less variance with regard to whether or not the Democrats offered candidates.[8] Since the measured dependent variables are dichotomous, we have employed probit analysis. The results for both offices are displayed in Table 2. We report results for three sets of independent variables: the two Republican party variables, the four demographic variables employed earlier, and the combined set of party and demographic variables. Because of the history of electoral weakness of the Republican party in the South, a dummy variable denoting a southern county has been included in each equation.

The probit results show clear and strong relationships between the county party variables and the probabilities that the Republicans will be running candidates for the U.S. House and the state

Table 1. Regression Analysis Predicting Republican Percentages of the Two-Party Vote

	1980			1982		
Variables	Congress	Senate	President	Congress	Senate	Governor
Population rank	.25	.31*	.18*	.15	–.05	–.11
Percentage high school graduates	.19*	.41*	.47*	.21*	.00	.21*
Percentage rural farm	–.05	.27*	.19*	.09	.25*	.40*
Percentage poor	–.13	–.10	–.17*	–.13	–.20	–.08
Republican structural strength	.21*	.07	.03	.06	–.10	.02
Democratic structural strength	–.08	–.02	–.18*	–.02	.11	.17
Republican campaign activity	–.06	–.05	–.00	.05	.11	.05
Democratic campaign activity	–.15	.00	–.07	–.18*	–.02	–.27*
Number of cases	154	110	182	160	118	137
R^2 (demographic variables only)	.07	.32	.33	.10	.08	.14
Increase in R^2 (with party variables added)	.04	.00	.03	.02	.01	.06
Total R^2	.11	.32	.36	.12	.09	.20

Note: Standardized regression coefficients are shown.

*Beta significant at .10.

legislature, relationships that withstand controls for local environmental attributes. As might be expected, the Republicans are less likely to offer candidates for these offices within the South and are more likely to field candidates in larger, urban counties. At the same time, the probabilities that a Republican candidate will be running are increased by the presence of a more structurally articulated and active local party. Structural strength is substantially more important than immediate campaign activity, a finding that suggests further that the running of a full slate of candidates may be a major electoral consequence of the "institution building" reflected in the structural variable.[9] These findings indicate that party organizations *do* "matter."

The potential significance of running a full slate of candidates for office may be substantial, with both direct and indirect benefits possible. A prerequisite for achieving majority status is to run a full complement of candidates. Without a candidate there can be no victory, even if a sudden shift in the electoral mood offers a "window of opportunity." Recent experience in the largest of the southern states illustrates this. Although the results of the 1984 Texas elections gave 98 of the 150 lower house seats to the Democrats, 83 of the Democrats were elected without Republican opposition. The Republicans were, in fact, victorious in 65% (28 out of 43) of the races they contested with the Democrats. While some of this pattern is due to decisions by the Republicans not to contest "hopeless" races, the simultaneous appearance of liberal presidential and U.S. Senate candidates at the top of the Democratic ticket also created the potential for substantial Republican gains, gains only partially realized at the state legislative level because the Republicans contested less than half of the races.[10]

In addition to the direct benefit of winning the contested office, there may also be "trickle-up" effects from lower races to higher ones. Looking again at our na-

(Table 1 Cont'd)

State Legislature 1984				
Lower House	Upper House	Congress 1984	Senate 1984	President 1984
.19	.34	.35*	.32*	.25*
.23*	.14	.24*	.09	.09
.21	-.11	.04	-.13	-.12
-.18	-.25	-.17*	-.23*	-.25*
.15	.04	.04	-.10	-.06
-.02	.17	.02	.00	-.23*
.04	-.00	.03	-.02	.00
.01	.09	-.12*	-.08	-.10
115	82	194	143	210
.14	.10	.16	.11	.13
.01	.03	.02	.02	.05
.15	.13	.18	.13	.18

tional county sample, regression analyses of both the 1984 presidential and congressional county results show statistically significant effects for the dichotomous Republican lower-office candidacy variables. Controlling for local environment and prior electoral propensity (measured by the Republican gubernatorial vote percentage in the previous election), a lower house (state legislative) candidacy was associated with increases in the Republican percentages of the congressional vote in southern counties of 9.3%. A similar analysis of the presidential vote showed increases of 4.8% and 3.1% for congressional and lower house candidacies, respectively. All of these effects were for southern counties only (i.e., were interactive), indicating that the electoral trickle-up bonus is obtained only in the context of a previously inactive or minority party situation. While our analysis has shown that there is little direct linear translation of county party strength and activity into county vote totals, there may be a significant indirect effect of county efforts on overall vote totals, an effect that is felt through the extent to which the party is locally active in offering a full slate of candidates for office.

Conclusions

Sundquist (1983) has noted that one of the direct electoral manifestations of a realignment is that a previously moribund party organization begins to contest elections. In recent times, one of the barriers the Republican party has faced in parts of the South has been the limited geographic penetration of the party beyond the rapidly growing metropolitan centers. So long as there are no local candidates to contest county and state legislative races, potential Republicans are likely to remain incomplete partisans, either independent or "dual party" identifiers (Hadley 1985). As local parties develop, perhaps in response to advances at the state level, these parties can offer an organizational base from which candidacies for these lower offices can be mounted. These *local*

Table 2. Probit Analysis of 1984 Republican Candidacy for Lower House of State Legislature and U.S. House in County

	Lower House of State Legislature			U.S. House		
Variables	Party	Control	Combined	Party	Control	Combined
Republican structural strength	.27	—	.22	.18	—	.32
	(.04)		(.02)	(.05)		(.04)
	7.09		9.50	4.00		8.55
Republican campaign activity	.05	—	.08	.06	—	.10
	(.03)		(.02)	(.03)		(.02)
	1.83		5.18	2.15		4.99
South	-1.72	-1.11	-1.28	-1.99	-.72	-1.27
	(.22)	(.11)	(.10)	(.83)	(.27)	(.22)
	-7.96	-10.03	-12.30	-2.38	-2.69	-5.87
Population rank	—	-.00	.00	—	-.00	.00
		(.00)	(.00)		(.00)	(.00)
		-4.55	1.21		-3.51	5.35
Percentage high school graduates	—	.07	.06	—	.08	.08
		(.01)	(.01)		(.01)	(.01
		9.21	9.18		7.20	8.53
Percentage rural farm	—	.01	.01	—	.12	-.00
		(.01)	(.01)		(.02)	(.01)
		1.71	1.96		5.23	-.29
Percentage poor	—	.07	.06	—	.05	.05
		(.01)	(.01)		(.01)	(.01)
		4.86	5.06		3.54	4.61
Constant	5.02	1.54	.18	6.48	1.68	-.69
	(.22)	(.58)	(.56)	(.83)	(.75)	(.70)
N	240	240	240	319	319	319
R^2	.61	.49	.61	.59	.64	.72

Note: Table entries are probit coefficients, standard errors of coefficients, and t-statistics. R^2 calculated according to the McKelvey-Zavoina formula.

candidacies, in turn, may complete the mobilization of potential adherents to the Republican party that has been started by the presence of Republican candidates higher up on the ballot.

We have shown that county-level party organizational strength and activity can make a difference for the electoral fortunes of the parties. The direct effects on vote totals are relatively slight. However, county-level party activity is related to the probability that a party will contest lower-level offices, an outcome that, in turn, is more generally related to the electoral fortunes of the party. Thus, the efforts of partisans to build and work through their county organizations appear to be part of the overall process of party competition and party system change.

More generally, these conclusions fit well with the state-level research of Cotter and his associates (1984), showing that party organizations do have an effect on electoral politics, that party organizations are indeed independent actors in the political process. This leads, further, to the argument that analysis of party system transformation cannot assume that all elements of parties are changing at similar rates or even in the same direction. Indeed, change in one dimension of party may stimulate compensatory change in the other dimensions, causing us to observe *negative* relationships between the condition of various party components. Further analysis should be mindful of the role that party organizations can play in counteracting declines in areas like mass attitudinal attachments. At the same time, political scientists must take care not to generalize "downward," assuming that what is true at one level of the political process is true elsewhere. The study of local party politics is not only useful for testing important hypotheses about party change, it also provides a more complete understanding of the multiple levels of political competition in U.S. party politics.

Notes

The party organizational data for 1980 are derived from the Party Transformation in the United States and the Institutional Party project, which was funded by the National Science Foundation (SOC 77-27020). We are indebted to the Party Transformation Study for making these data available. North Texas State University, the University of Texas at Dallas, and the University of Houston's Center for Public Policy provided support for the collection of parallel data in 1984. Electoral data were supplied by the Inter-University Consortium for Political and Social Research (ICPSR). Additional support was provided by a summer research grant from Loyola University, Chicago. Colleagues who have made helpful contributions include Paul Herrnson, Samuel Eldersveld, and Alan Gitelson.

1. A larger body of research has examined the broader question of the connection between campaign activity and electoral events. Much of the recent work has focused on the effects of campaign spending (e.g., Caldeira and Patterson 1982, Jacobson 1980) or mass media advertising (e.g., Hofstetter and Buss 1980). An earlier line of research sought to identify the relative benefits of various types of activities, such as personal contact techniques (such as canvassing) versus newspaper ads or billboards (e.g., Eldersveld 1956, Kramer 1970-71).

2. It is unclear what analyses of surviving big city political organizations indicate about partisan politics in the suburbs, the Sunbelt, and the Mountain states. Also, many of these studies utilize pre-1964 data, a time frame that predates a period of mass attitudinal transformation and the appearance of many of the technological and statutory changes thought to be so destructive of the parties' influence.

3. In using the county party organization as our basic unit of analysis we are following Crotty (1971), Beck (1974), and Gibson and Smith (1984), among others. While the county represents a genuine level of activity for both major parties (see Gibson et al. 1985), it should be noted that both parties pursue activities simultaneously at multiple levels—nationally, by state, by election districts, by media markets, and so on. Thus, analysis of party activity at any one level is likely to offer only a partial picture of the electoral roles of political parties.

4. Electoral returns from 1980 and 1982 are from the ICPSR data base; 1984 returns were obtained directly from the states. Since the ICPSR data base does not include state legislative races, these data are available only for 1984. Gubernatorial returns have been analyzed only for 1982 because of the small number of states electing governors in 1980 and 1984. Counties with uncontested races have been excluded from this portion of the analysis for that office in the year(s) for which they were uncontested.

5. Overall level of campaign activity was measured by the number of activities (from a list of 15) in which the party organizations engaged. Structural strength was measured by a Guttman scale based on nine indicators: (1) constitution and by-laws, (2) complete set of officers, (3) a chair devoting at least six hours per week to party business during election period, (4) a chair devoting at least six hours per week to party business during nonelection period, (5) bimonthly meetings, (6) year-round office, (7) telephone listing, (8) full- or part-time staff, and (9) formal annual budget (see Gibson, Frendreis, and Vertz 1989).

6. The use of national cross-sectional data raises the possibility that local variations in party strength are masking the relationship between party activity and voting results. Even with demographic controls, different baselines of normal voting patterns may obscure whatever marginal electoral returns on party activity exist. To address this possibility, we replicated this analysis with a dependent variable in which national and local voting trends had been removed. Using the Stokes (1965) variance components procedure, we extracted the national and state long-term and short-term effects from the 1980, 1982, and 1984 county vote totals and regressed the remaining vote component (the local component of the Republican two-party vote percentage) on the same set of party and demographic variables. The results do not vary. (For details on this procedure, see Vertz, Frendreis, and Gibson 1987.)

7. Elsewhere we note that redistricting may increase the local component of electoral variance (Vertz, Frendreis, and Gibson 1987). This decennial disturbance may create opportunities for some local organizations to have a greater-than-normal effect on local voting patterns.

8. For all 240 counties for which state legislative data are available, Democrats ran unopposed in 50 counties, Republicans ran unopposed in 25 counties, and candidates from both parties competed in 165 counties. For the 319 counties for which congressional data are available, Democrats ran unopposed in 23 counties, with all other counties contested. The Republican candidacy variable has a value of zero for the counties in which a Democrat ran unopposed and one for the other counties.

9. Obviously, some of this relationship might be reciprocal, i.e., local parties become more active as a result of local candidacies. In additional analyses (not reported here), the relationship between the party variables and 1984 electoral outcomes persists even when variables are included to control for previous level of electoral outcome (e.g., 1980 congressional vote). This lends support to an inference that at least a substantial portion of the causality is from party strength to electoral activity.

10. We also speculate that some voters may be reluctant to vote a straight party ticket when there are a significant number of vacancies in the party list.

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THE STRATEGY OF CANDIDATE AMBIGUITY

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If candidates are uncertain about the policy position preferred by the median voter and therefore face the risk of stating an unpopular position, in equilibrium both candidates may prefer to make their positions ambiguous rather than to specify them. The incentives to be ambiguous are further increased if the position announced by one candidate allows the other candidate to estimate the preferences of the voters better.

The standard one-dimensional spatial model of competition among candidates (Downs 1957; Hotelling 1929) supposes that each candidate specifies a position on the issues. In many political campaigns, however, candidates choose to leave their positions ambiguous. I show conditions under which such a strategy will be preferred by a candidate.

Shepsle (1972) shows that ambiguity pays if voters are risk-loving. It is no surprise, however, if candidates are ambiguous when voters enjoy risk. The point made here is that ambiguity may arise even if all voters prefer that there be none. Enelow and Hinich (1981) also study voter uncertainty in a one-dimensional model, but they take uncertainty as given rather than as a consequence of candidates' strategies. Other authors (Bernhardt and Ingberman 1985; McKelvey 1980; Zeckhauser 1969) allow candidates to adopt ambiguous positions. These models assume that candidates have perfect information about the preferences of the voters, and therefore conclude that ambiguity is never worthwhile. The analysis to follow yields different results because it rests on the assumption that candidates have only imperfect information about the preferences of voters. A candidate who specifies a position therefore runs the risk of specifying an unpopular one. (Calvert [1985] and Coughlin and Nitzan [1981] make similar assumptions about candidates, but their focus is on the positions announced, rather than on whether to announce them at all.)

Ambiguity As a Dominant Strategy

Let p denote a voter's ideal point: the position the voter likes best. A voter's utility from the election of a candidate with position s is $u(p, s)$. Utility is at a maximum for $s = p$; $\partial u(p, s)/\partial s$ is negative for $s > p$ and positive for $s < p$. This means that the further away in a given direction a candidate's position from the voter's most preferred point, the less attractive that candidate is to that voter. Only two candidates vie for office in any election. A person votes for the candidate whose expected position provides the greatest utility.

Under these assumptions the winner in

any election will be the candidate who wins the support of the median voter. The analysis can therefore focus on the median voter's preferences. Arbitrarily scale the median voter's utility function so that $p = 0$. Then the median voter's utility from seeing the election of a candidate with position s is $u(s)$

Consider two types of uncertainty. A voter may be uncertain about the position of any candidate, and a candidate may not know the ideal point of the median voter. For example, suppose the only issue in the election is the size of the budget and that candidate C in fact favors a budget of a hundred million dollars. A voter, however, may not precisely know C's position. The voter may think, for example, that with a probability of 30% candidate C favors a budget of less than a hundred million dollars. This uncertainty is treated here as exogenously fixed, though a more general model could allow a candidate to affect the degree of this uncertainty by advertising or by explaining his or her positions in certain ways.

Similarly, a candidate may be uncertain about the median voter's preferences. For example, with probability one-half the candidate's best estimate of the median voter's ideal point may lie to the left of the median voter's actual ideal point. Candidates may not know the position preferred by the median voter for several reasons. Actual turnout can be a random variable. Therefore, the identities of the voters in general, and of the median voter in particular, may be unknown at the time candidates announce their positions. Or conditions may change between the time at which a candidate announces a position and the time at which the election is held. A deteriorating economy, foreign crises, financial panics can all unexpectedly alter the preferences of the voters. This uncertainty is also taken to be exogenously fixed, perhaps at the optimal level that a candidate would choose, given that resources devoted to gathering information about voters cannot be devoted to campaigning in other ways.

Note that although no candidate knows the position of the median voter, such a voter does exist; and the outcome of the election can be described by that voter's preferences. Moreover, because the candidates do not have perfect information, they can have differing notions about the position that would win the most votes. In contrast to solutions obtained under perfect information, the candidates may adopt positions that are not identical. (See Ferejohn and Noll 1978 for an extensive discussion of this point.)

More formally, let the median voter believe that with probability $V(s)$ the candidate's position is to the left of point s. The corresponding probability density function is $v(s)$. Let $C(s)$ be the probability that the distance between the candidate's announced position and the median voter's ideal point is s. (This distance is positive if the candidate's announced position is to the right of the median voter's ideal point and negative if it lies to the left.) The corresponding probability density function is $c(s)$. Note that if the candidate had perfect information, $C(s)$ would collapse around zero. For simplicity, suppose for now that these functions are identical for the two candidates. (To remember the notation, $v(s)$ represents the *voter's* beliefs, and $c(s)$ the *candidate's* beliefs.)

The purpose of this section is to determine a Nash equilibrium set of strategies for the two candidates. Each candidate can either be ambiguous or else can specify a position. Note that the optimal strategy for a candidate who specifies a position (but does not know the position, if any, that the other candidate adopted) is to announce the position he or she believes represents the ideal point of the median voter.

Let u_A be the median voter's expected utility from the policy that would be implemented by a candidate with an ambiguous position. By definition,

$$u_A = \int_{-\infty}^{\infty} u(s)v(s)ds.$$

To each value of u_A there correspond values of s (s_L and s_H, where $s_L < 0$ and $s_H > 0$) such that $u(s_L) = u(s_H) = u_A$. Since the voter, by the definition of s_L and s_H, is indifferent between an ambiguous candidate and one who specifies a position at either of these points, the voter prefers a candidate who specifies a position *inside* the interval (s_L, s_H) over a candidate who is ambiguous. The probability that the median voter prefers the unambiguous over the ambiguous candidate is therefore $C(s_H) - C(s_L)$.

For a benchmark case assume that $V(s)$ and $C(s)$ have identical uniform distributions. In particular, let $c(s) = v(s) = 1/2$ on the domain $(-1, 1)$. Let $u(s) = -|s|$; this represents a risk-averse utility function. If a candidate adopts an ambiguous stand, the expected utility of the median voter from the policy that would be implemented by that candidate is

$$\int_{-1}^{1} -1/2\ |s| ds = -1/2.$$

If, instead, the candidate specified a position, he or she will specify with probability one-half that is more than half a position unit away from the median voter's ideal point.

Consider, then, two candidates, and suppose candidate A adopts an ambiguous position. By assumption an ambiguous position generates uncertainty in the mind of the voter, which is described by the distribution $V(s)$. If candidate B also adopts an ambiguous position, the median voter will be indifferent between the candidates, so that candidate B will win with probability one-half. If, instead, candidate B specifies a position, he or she will win if the position taken is within half a unit of the median voter's ideal point. Since, by assumption, with probability one-half candidate B's announced position would lie within half a unit of the median voter's ideal point, candidate B will win with probability one-half. Thus, each candidate has the same chance of winning by specifying a position as by being ambiguous, regardless of the strategy the other candidate uses. A Nash equilibrium can have candidates be either ambiguous or not.

With this result as a benchmark, it is possible to consider variations in the assumptions. First, suppose each voter's utility function exhibits more risk aversion than assumed above. This decreases a voter's utility from the election of the ambiguous candidate and therefore increases the probability that the voter will prefer the candidate who specifies a position. A Nash equilibrium would have both candidates specify their positions.

For example, let s take only the values $-k$, -1, 0, 1, and k, where $k > 1$; and let $c(s) = v(s) = 1/5$ for each of these values. Let the median voter's utility function be $-s^2$. The voter's expected utility from seeing the election of a candidate with an ambiguous position is $-2/5(k^2 + 1)$. With probability three-fifths a candidate who specifies a position chooses a position within one unit of the median voter's ideal point. If $|-1| < |-2/5(k^2 + 1)|$, that is, if $k > \sqrt{3/2}$, a candidate who specifies a position will defeat an ambiguous candidate with probability of three-fifths. If, however, $k < \sqrt{3/2}$, a candidate who specifies a position will defeat an ambiguous candidate only if the former announced a position coincident with the median voter's ideal point. Since by assumption this occurs only with probability one-fifth, ambiguity would be the dominant strategy. Thus, a Nash equilibrium could have both candidates ambiguous, even though voters are risk-averse.

Suppose next that the functions $C(s)$ and $V(s)$ differ—say, that $V(s)$ is more tightly centered about the ideal point of the median voter than is $C(s)$. Under these conditions a candidate would find it more advantageous to adopt an ambiguous position. To see this consider a modification of the earlier example. Suppose $V(s)$ is uniformly distributed on the domain $(-1, 1)$, whereas $C(s)$ is uniformly distributed on the larger domain $(-2, 2)$. The probability that a candidate who is ambiguous will defeat one who states a position is

$$1 - \int_{1/2}^{1/2} C(s)ds = 1 - 1/4 = 3/4.$$

A Nash equilibrium set of strategies would therefore have both candidates ambiguous, each winning with probability one-half. A similar argument demonstrates that if $V(s)$ is less tightly concentrated than $C(s)$ about the median voter's ideal point, the Nash equilibrium can have each candidate specify a position.

Sequential Announcements

The previous section assumed that candidates choose their strategies simultaneously or without knowing what the other candidate has done. Suppose, alternatively, that candidate A announces a position first. This position is the candidate's best estimate of the median voter's ideal point. The other candidate, candidate B, would then be able to use information about candidate A's estimate to estimate the true median position better. I shall demonstrate that the candidate who announces a position last can better determine the true preference of the median voter and is therefore the one most likely to win.

Call m the true ideal point of the median voter. Let $F(x)$ be the proportion of voters with ideal points to the left of x. By the definition of m, $F(m) = .5$. Let candidate A believe that the median voter's position is a and suppose candidate A announces that position. Since candidate A lacks perfect information, with some nonzero probability he or she errs in estimating. Similarly, define b as candidate B's initial estimate of the median voter's position. Now it is fair to assume that both a and b are unbiased estimates, so that on average both $F(a)$ and $F(b)$ equal 1/2; if both candidates announced these respective positions, they would have equal probabilities of winning. Candidate B, however, can do better, by choosing a position just to the left of a if $b < a$ and just to the right of a if $b > a$.

Consider the situation where b is less than a. Both candidates believed that the median is to the left of a. By assumption, these estimates are unbiased and independent. The probability, therefore, that the true median is greater than that guessed by both is only one-fourth. In other words, if $b < a$, the probability that the ideal point of the median voter is to the right of point a is one-fourth. The probability that it is to the left of a is three-fourths. Candidate B therefore expects to attract the most votes by announcing a position just to the left of a. The candidate's probability of winning is then three-fourths. If A announced first and B's estimate, b, was greater than a, candidate B would choose a position just to the right of a and again win with probability three-fourths.

In a Nash equilibrium, therefore, no candidate would wish to be the first to announce a position (unless, of course, the candidate could costlessly change the position after it is announced; issues of credibility, however, could preclude such a strategy). In equilibrium neither candidate announces a position, or else a candidate announces a position at such a late date as to preclude the opponent from effectively publicizing a position. Indeed, wishywashiness on the part of candidates and an unwillingness to state their positions are not unheard of in the political arena.

Summary

Candidates may find ambiguity a dominant strategy for two separate reasons. Ambiguity can be explained in terms of vote maximization and need not only apply to risk-losing or irrational voters. These results are disheartening: although voters could make better choices if candidates always specified their positions, candidates do not always have incentives to do so.

Note

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ASYMMETRIC INFORMATION AND THE COHERENCE OF LEGISLATION: A CORRECTION

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In the September 1987 issue of this Review, *we developed a game-theoretic model of committee or legislative decision making, showing the extent to which members may have incentives to share, or conceal, information. Because the formal analysis and statement of results for this model were in error, we offer this correction.*

In an earlier article (David Austen-Smith and William H. Riker, "Asymmetric Information and the Coherence of Legislation," *American Political Science Review* 81 [1987]: 897–918) we examined a model of committee decision making in which participants possess differing information about the likely consequences of any decision. In such an environment committee members have conflicting incentives with respect to information sharing. On the one hand, there is an incentive to share information to obtain better estimates as to which bills are most in their particular interests. On the other hand, they have an incentive to conceal private information to manipulate the decision-making process in their favor. In our model information could be transmitted in three ways, two of which potentially affected the final committee decision. The first stage of the decision-making process was debate. In debate, each individual simultaneously offered a noisy (but unbiased) estimate of private information to the committee. The second stage was agenda setting. Having heard the debate, each individual simultaneously proposed an alternative (a bill) for selection by the committee. The agenda was this set of proposals. In the third stage, individuals simultaneously reported all their pairwise votes over alternatives on the agenda. The final decision was assumed to be the Condorcet winner among these alternatives. Information might therefore be revealed in all three stages, but any new data generated during voting could not affect the committee decision. Our principal result was that even though debate messages were constrained to be unbiased (so lying was not permitted) and individuals were distinctly risk-averse, not all information germane to the committee decision would be revealed by the agenda-setting stage. Although perhaps not surprising, this conclusion implied that the final committee decision was typically not the Condorcet winner among the preferences that individuals would have if data were fully shared.

Unfortunately, the formal analysis and statement of results were flawed. The first mistake was our claim that sincere voting relative to individuals' information at the time of the vote necessarily constituted equilibrium behavior. This is false, even with complete information. The second, more serious, mistake was our claim that at the agenda-setting stage, all individuals would propose their most-preferred bill relative to their information at that stage. This second claim was predicated on the first, and failed to account for how others would interpret any given individual's proposal. In particular, given that everyone other than legislator A believes that A's proposal is the most-preferred bill, the legislators will update their own information to reflect the data revealed through the proposal. But knowing this, if A believes that his or her most-preferred bill could not be the final committee decision, A can potentially offer an alternative distinct from A's most-preferred and "move" others' beliefs in a way that is ultimately to A's benefit. Therefore, typically, offering sincere proposals is not equilibrium behavior. Because rational choices in the debate stage are predicated on anticipated subsequent behavior, it now follows that our results on information sharing in debate are suspect.

We now argue that our substantive conclusions are nevertheless correct, although our formal proposition (p. 902) is not. To do this we develop a somewhat different model from our original specification. There are four differences from the original model: (1) instead of assuming the random variable that connects bills to consequences to be normally distributed, we now assume it is described by a Bernoulli distribution; (2) individuals in debate are no longer constrained to offer unbiased estimates of their private information—lying is permitted; (3) at the voting stage individuals are now constrained to offer single-peaked orderings of alternatives on the agenda (but these orderings are not necessarily their true preferences); (4) the committee comprises exactly three legislators. Consider each of these in turn.

Bernoulli distribution. With a normally distributed random variable, the private information of any legislator was only constrained to be some point on the real line. Thus, a legislator's most-preferred bill, given his or her private information, could likewise be anywhere on the real line; and this gives rise to considerable technical complexity in solving for equilibrium proposals. Although such complexity does not disappear by assuming that the random variable can only take two values, the fact that a legislator's most preferred bill can be in at most a finite number of places (despite the proposal's being unconstrained) does not permit a more direct approach. Moreover, there is still sufficient richness in the construction to make our substantive problem nontrivial.

Lying is permitted. Allowing legislators to lie in debate is a considerable generalization. We had discussed how the assumption of unbiased messages was strong and ultimately undesireable. Here, we relax the assumption.

Single-peaked orderings. The specification of the voting stage was intended to help "isolate the strategic aspects of information transmission" in debate, and we claimed that "the voting mechanism effectively give[s] legislators dominant proposal [and voting] strategies" (p. 902). This, as noted above, turns out to be incorrect. Although restricting voting strategies to be single-peaked orderings of the alternatives does not induce dominant voting strategies or generally guarantee sincere voting, it makes equilibrium behavior at the voting stage much easier to identify. As such, the restriction does help "isolate the strategic aspects of information transmission" in debate.

Three legislators. Nothing in our results seems to hinge on assuming only three legislators. The additional analytical complexity of introducing more individuals, however, is considerable.

With these changes to the model, we establish the following formal results. Let b^* be the committee decision if all individuals' information was fully shared in debate, and let b be the actual committee decision.

CLAIM 1. *There exists a distribution of committee members' ideal points in the* *space of consequences and an equilibrium to the decision-making game such that (a) if there were* no *debate stage, all private information would be revealed through agenda setting and the probability that $b = b^*$ is strictly greater than zero and strictly less than one and (b) if there is a debate stage, all private information is revealed in debate, and the probability that $b = b^*$ equals one.*

CLAIM 2. *There exists a distribution of committee members' ideal points in the space of consequences and an equilibrium to the decision-making game such that (a) the individual with the median ideal point in consequences and the individual on one side reveal their information in debate, but the individual on the other side conceals his or her data in debate and (b) no additional information is revealed at the agenda-setting stage and the probability that $b = b^*$ is strictly greater than zero and strictly less than one.*

CLAIM 3. *There exists a distribution of committee members' ideal points in the space of consequences such that in any equilibrium to the decision-making game (a) at most the individual with the median ideal point in consequences reveals his or her information in debate and all others conceal their private data and (b) no new information is revealed at the agenda-setting stage and the probability that $b = b^*$ equals zero.*

(The formal analysis supporting these claims is available from the authors on request.)

Claims 1 and 2 reflect possibilities identified in our original proposition. Claim 3 reveals a possibility ruled out by that proposition. Thus, the substantive interpretations of our proposition discussed in the earlier piece remain unaffected. In particular—and in continuing contradiction to some extreme claims of liberalism—the interpretation stands that freedom of debate does not imply the revelation of all of the information necessary for socially optimal decisions. However, it is worth making two remarks. First, the claims here are somewhat different in character from, and in some senses more definitive than, the original proposition. The proposition is essentially a characterization result. We asserted that in any equilibrium the median individual in consequences and half of the rest of the committee would *always* share all of their private information, whereas the others may or may not share some private data depending on the distribution of ideal points. In contrast, the claims made here are existence results. We assert the existence of distributions of ideal points for which there are equilibria with the relevant properties. There may be other equilibria for any given distribution of ideal points (indeed, there is always an equilibrium in which no one ever says anything in debate). Second, one legislator, *at most,* proposes sincerely at the agenda-setting stage (the median in the space of consequences) and then only in claims 1b and 3.

We thus have reached some assurance that situations exist (claims 2 and 3) in which, for certain, the outcome is not coherent, as defined in the original paper.

Note

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CONTROVERSY

POLITICAL CULTURE AND POLITICAL CHANGE

In "A Culturalist Theory of Political Change" in the September 1988 issue of this Review, *Harry Eckstein argued that "a cogent, potentially powerful theory of political change can be derived from culturalist premises." But Herbert Werlin finds Eckstein's effort to accommodate culture theory to political change unsatisfactory. Werlin argues that politics in the sense of political engineering, rather than cultural changes, mainly accounts for transformations in political life. Eckstein responds, arguing that the political methods for inducing change are themselves culturally conditioned.*

Harry Eckstein (1988) correctly criticizes cultural theorists for coping inadequately with political change. However, his effort to revise the culturalist theory of change must be seen as unsatisfactory.

First, Eckstein continues to defend the "postulate of cumulative socialization," namely, that early learning is a filter for later learning, is harder to undo, and forms a coherent whole (p. 791). This is based on Freudian theory that has come to be discredited. E. Fuller Torrey, a psychiatrist and anthropologist, notes that there is no evidence that early childhood experiences are crucial in determining later behavior or that there are distinct stages of personality development (1986, 75). Rogowski (1974) questions, not only the extent to which early learning conditions later learning but also the durability of early learning and its coherence. He points out that there is no agreement, as to which cultural values are most durable or basic, that learned behavior can be distinguished from other behavior, and that norms and values need to be congruent with those stressed by the political system.

Second, Eckstein argues that more modern societies are more flexible than more traditional societies (p. 795) and thus can accommodate rapid situational and structural change. However, the rapid rise of nationalism in Africa and Asia would have been impossible without an equally rapid process of Westernization (Emerson 1960). The impact on African traditional culture of urbanization, capitalism, mass party systems, and technological changes has been well researched. The same acculturation process has been observed in Latin America and Asia (Southall 1973).

Third, the more rapid the pace of political transformation, Eckstein insists, the more dysfunctional the consequences are likely to be, leading to political extremism, ritual conformity, retreatism, rebellion, intransigence, regression, or other unintended outcomes: "I suggest the expectation that the long-run effects of attempted revolutionary transformation will diverge considerably from revolutionary intentions and resemble more the prerevolutionary condition of society" (p. 800). But none of these consequences are apparent as a result of the extraordinary cultural transformations that have occurred in Asia's "newly industrialized countries," particularly Taiwan and South Korea.

As Taiwan's per capita annual income has grown from $48 in 1952 to more than $5,000, many aspects of its way of life have changed. Between the early 1950s and the early 1980s, Taiwan went from a largely agricultural to a largely industrial country (Harris 1986, 46–47). The progress of South Korea may even be more remarkable, considering the devastation of the Korean War, going from a 1957 per capita annual income of $67 (in current dollars) to about $2,300 by 1986: "Today, the visitor who had been absent for two decades, traveling in almost any portion of the nation, would have difficulty recognizing Korea, either urban or rural" (Steinberg 1985, 20). While there has been some political unrest in both Taiwan and South Korea, there is no evidence of traumatic cultural discontinuity.

Culturalists might argue that the economic success of Taiwan and South Korea is derived from basic Confucian cultural values that have remained unchanged, particularly respect for authority, education, and meritocratic administration. These cultural values, however, were not evident during the Chinese cultural revolution that had such a terrible effect from 1950 to 1980. While Chinese culture may have partly survived this revolution, it cannot be used to explain Chinese history. Taiwan's Kuomintang and the Chinese Communist party, Liu (1987) points out, grew out of the same cultural tradition but have moved in quite divergent political directions, with profound implications for all aspects of life. Townsend (1974, 2–3) notes, in regard to China, "At various times it was thought that China could never gain true unity and independence; that Chinese Communism could never become a significant political force; that China would necessarily develop as a pro-Western power; that the Chinese peasant could not be collectivized; that the Sino-Soviet alliance could not be broken; that the authority of the CPP could not be seriously threatened from within." While judgments and predictions may be possible, he concludes, they require "an expanded vision of what is politically possible."

The primary thrust of Eckstein's article is to suggest that while slow cultural change is normal, understandable, and perhaps useful, rapid cultural change is unusual, unsustainable, and dangerous. This conclusion is discouraging for those of us working on foreign aid projects requiring swift behavioral modification because it is often combined with the assertion that there are serious cultural barriers to rapid socioeconomic development. Three recent books (Harrison 1985, Hyden 1983, and Pye 1985) suggest that these barriers may be almost insurmountable. They follow a long line of culturological literature, proclaiming that "the behavior of peoples is determined by their respective cultural traditions" (White 1969, xxiv).

In regard to Latin America, Harrison points to supernatural belief systems, authoritarianism, corruption, absence of trust and cooperation, patrimonial relationships, and social rather than legal sanctions. Hyden stresses Africa's "economy of affection"—the loyalty of people to groups defined by kin, community, and religion, thereby undermining loyalty to the state and its institutions, laws or rules, and administration. Pye finds a far greater degree of paternalism, authoritarianism, ethnocentricity, selfishness, and xenophobia in Asia than in the West, despite cultural differences within these parts of the world.

While recognizing the reality of these cultural barriers, we can question their intractability. I will suggest some problems with the culturalist case that remain unresolved in the Eckstein article.

Discontinuity. A number of scholars once believed that there was something in the German national character (an authoritarian personality) that accounted for the rise of Nazism (Deutsch and Breitling 1963). However, gloomy predictions about the future of democracy in West Germany proved unsubstantiated (Verba

1965). Eckstein questions the possibility of revolutionary cultural transformation (pp. 799–801), suggesting that the shift in West Germany from a totalitarian to a democratic orientation between the 1930s and the 1960s is a "deviant case." However, this case may not be so deviant. The ability of people to drop quickly or add cultural attributes in response to new situations has been frequently observed, particularly in the United States. While immigrants to the United States maintained much of their cultural background, "paupers became shopkeepers, and the sons and daughters of peasants bound for centuries to slivers of poor soil turned into clerks and nurses and accountants and schoolteachers and druggists and cab drivers and lawyers and doctors" (Cooke 1973, 300–301).

Rapid cultural change does not require a political revolution, as suggested by Eckstein. Within a relatively short time at the end of the eighteenth century, Josiah Wedgwood had turned a traditionalistic work force (used to drinking on the job, working flexible hours, and working at their own discretion) into an efficient, diligent, and reliable work force (Langton 1984). A more recent example can be found along the Mexican border area, where Mexican workers, thought to be lackadaisical, have proved highly productive in *maquilas*, which, supported by both Mexican and U.S. governments, have been able to compete with East Asian and European factories (Rivera and Goodman 1981).

Causation. While culturalists generally see culture as a "persistent pattern of social behavior," they disagree about the causes of this behavior and the priority of the various kinds of factors: ideological (prejudices, fears, values, beliefs, goals), socioeconomic (class structure, interethnic rivalry, standard of living, occupation, technology, geography), historical (impact of conquest, religion, colonialism, nationalism, industrialization, war, revolution, international relations), and political factors. The uncertainty about the causes of culture motivates its use as a catch-all explanation, much as the psychosomatic one in medicine (Papanek 1988). As such, it loses its explanatory value in regard to national development. Chalmers Johnson, a leading U.S. expert on Asia, notes that "Japan's culture was at least not an obstacle to development—adding, "But then neither was it an obstacle to militarism, elitism, emperor worship, colonialism, or many other things that the Japanese prefer to forget" (quoted in Redding 1988, 100).

Definition. Eckstein prefers the definition, "Culture is the distinctive, variable set of ways in which societies normatively regulate social behavior" (p. 803). The question remains, Where does politics fit in? While leaders are guided by societies, they also can manipulate them.

If it is true, as Eckstein asserts, that this definition of culture is "justified by usage in political science" (p. 803), it may be because of the tendency to confine politics to conflict and competition among individuals and groups. Interest group pluralism goes back to an influential body of literature, particularly Harold Lasswell's *Politics: Who Gets What, When, How* (1936). To see politics in this way is as misleading as to see the Olympics as nothing more than competing individuals and teams, neglecting the organizational arrangements and consensus on rules, guidelines, judging, and participation that are essential for what takes place. Our conception of politics is most meaningful, Wolin (1960, 434) reminds us, the closer we keep to the word's Greek derivation from *polis* (community): "Political theory must again be viewed as that form of knowledge which deals with what is general and integrative to man, a life of common involvements."

Politics—the relationship of leadership to followership—affects culture, just as it is affected by culture. We can, for example, organize or structure a course in such a way that every student is likely to cheat on the final examination. Yet, we can also reduce corruption. While corruption may have traditional roots, it was much more controlled under colonialism than since then in India and Nigeria (Dwivedy 1972, Ekiyor 1988). Corruption has also been effectively discouraged in Botswana, Malawi, and the Ivory Coast, where leaders have seriously fought against it. On the other hand, corruption has flourished where leaders have used it to survive.

Cultural barriers to development are certainly difficult to overcome. Only 11% of the U.S. Agency for International Development (USAID) projects and 44% of the World Bank's projects have survived termination of foreign aid (World Bank, Operations Evaluation Department 1987, 20; Werlin 1987, 41). Montgomery (1987), in a study of management in nine southern African countries, found many of the complaints about African managers (concern with personal power and privilege, indifference to policy issues, distrust of the private sector, and fear of risk taking and experimentation) to be valid. Yet he also raises the question whether such factors as ethnicity are behavioral or systemic.

The World Bank places the blame for the failure of 74% of its rural development projects largely on deficiencies in project design and implementation (World Bank, Operations Evaluation Department 1988, 27–28). In 23% of its projects for the five fiscal years 1979–83, lack of government support was judged the most important adverse factor. Thus, the World Bank sees politics, rather than culture, as undermining these projects.

While cultural barriers to development are undoubtedly a reality, their immutability is a myth. Ultimately, politics is more powerful than culture. This can be seen by looking at some of the foreign aid success stories where significant behavioral modification was brought about by strong leaders serious about development and by well-established organizations (Paul 1986). In each case, success resulted when the quality of political relationships (political software) was high enough to enable the institutional structures (political hardware) to function properly. Political software requirements include

1. *Combinations of centralization and decentralization.* Leaders delegate responsibility to subordinate subdivisions or organizational units without losing supervisory and correctional potential. Each step toward decentralization must be followed by steps to improve managerial supervision, enforcement of regulations, and hierarchical control.

2. *Boundary maintenance and control.* Certain positions (e.g., judges, evaluators, auditors, and ombudsmen) are protected without diminishing the ultimate responsibility of top political and administrative leaders for results. Experts must also be properly organized, assisted, motivated, supervised, protected, and controlled.

3. *Soft-hard power integration.* Leaders maximize the use of soft forms of political power (influence, persuasion, incentives, encouragement), as against hard forms (coercion, punishment, threats). Leaders must integrate the two forms of power in motivational and constructive ways.

4. *Legitimacy.* Forms of popular or subordinate participation are encouraged without undermining managerial and leadership capacity. Social or ethnic discrimination must be avoided. Communication from below must be encouraged, including conflict and dissent, without diminishing the capacity of the leaders for policy implementation, interest aggregation, and rule adjudication.

5. *Flexibility.* Objectives are agreed on, allowing a variety of approaches to be used to attain these objectives. Creativity must be encouraged without diminishing predictability, accountability, and enforceability.

When political software requirements

are met, political culture can be changed quickly and effectively (Werlin 1986). In El Salvador's Fundacion Desarroyo project, people were organized into mutual help groups to build the infrastructure and foundations for low-income housing. They repaid loans and cooperated with management to an extent that surprised and delighted World Bank officials. In Egypt's USAID-supported Helwan project, one of the community associations gained enough support from residents to rely on them to sweep the streets voluntarily rather than pay people to do it. In Gujarat, India, low-income, self-employed women were trained by the Self-Employed Women's Association (an organization led by Ela Bhatt, a dynamic local leader) to overcome their fear of the banking system and to undertake new types of production, storage, marketing, and financial management. These cases, as well as many others, show that people will respond to favorable political relationships under otherwise adverse circumstances.

The most successful foreign aid projects combine incentives and penalties to motivate people to improve their lives. Such projects have been introduced by the World Bank in the Calcutta Metropolitan District and by USAID in rural Egypt. They provide initial funds to civic units for certain agreed-on programs, with renewed funding dependent upon specific objectives being met. The Inter-American Foundation uses this approach, carefully selecting nongovernmental proposals, monitoring their progress, and continuing to fund only the more successful. The result might be called "political elasticity," simultaneously expanding opportunity and control (Werlin 1988).

We end with warning as well as hope. Political culture an retrogress as quickly as it can progress. The neighboring countries of Malaysia and Singapore provide examples of each. In Malaysia, interethnic or interracial conflict (particularly, between Malays and Chinese) has intensified since independence; whereas in Singapore, it has diminished. In each case, politics, rather than culture, has been the guiding force. We must not allow culturalists to diminish the importance of politics without a strong counterargument.

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In my article on a culturalist theory of political change I did not argue that such a theory is valid (which remains to be seen), but that it is *possible* to construct such a theory without cheating. I did this because arguments that culturalists could not theorize about political change without undermining their own theoretical perspective existed and had a cogent basis—namely, that the postulates of political culture theory did indeed make continuity the logical expectation. I also tried to spell out what such a theory would be in regard to various kinds of change and what sort of changes it could not handle.

Of course, this was no mere intellectual exercise. At a minimum, I wanted to show that political culture should be regarded as a serious player in the game of explaining political change. No doubt, also, I lean toward culturalist theory, though hardly as dogma (see Eckstein 1974). Still, Herbert Werlin's criticism of my article is based on a mistaken, if understandable, reading of its nature. However, that does not preclude examining the cogency of the attack.

Let us leave aside Werlin's minor points: that I am a Freudian and that I make a dubious distinction between *tradition* and *modernity* (in fact, although I use the words, I have always been concerned with "development" as a continuous dimension, not a dichotomy; see, esp., Eckstein 1982). The crucial argument here is one in favor of "political engineering." Incremental (gradual, limited) change does not seem to be in question, since it is the sort of change least problematic for culturalist explanation. Rather, the point is that political change that is rapid, broad-scale, and nonrevolutionary *can* be politically wrought—if only the right "tools" are used. (If I have read correctly, that makes most of Werlin's examples of change not really pertinent.) True, USAID and World Bank projects hardly have had an impressive record of success (although their objectives rarely, if ever, have approached broad transformative political change). But that, my critic argues, is not due to cultural inertia but to engineering errors: bad management or faulty project design and implementation and lack of governmental support. This view is only to be expected from agencies and officials engaged in bringing about meliorative change.

The activity is laudable. But just for that reason it should be pursued without illusions. Engineers achieve results effectively by adapting well-established theories (or in lieu of theory, reliable experience) to the conditions with which, willy-nilly, they must work. It is both possible and likely that culture is one of the givens to which social engineers must adapt, whether in management, project design, implementation, or obtaining governmental support. It is also likely that culture will rule out certain objectives in certain contexts. If social engineers are serious, then, they should welcome culturalist theories as possible aids to achieving effectively results and defining realistic, however limited, goals and means.

But the argument here seems to be that "politics" can achieve just about *any* effects if only the right methods are used. What, then, are these methods? They are, without exception, just the sort of things that are the essence of political cultures! We are told that one always needs a right balance between decentralized responsibility and central supervision and control (whatever such a balance may be). One of the fundamental differences among political systems and their cultures concerns precisely this variable. Could one really, for instance, imagine any major modification in Switzerland's highly decentralized cantonal system, simply as a tool to achieve some end? After all, attempts to form a centralized union have failed since 1481 except for the very gradual formation of a weak "state" out of various alliances and confederations of "sovereign" cantons, divided (culturally) by language and religion, as well as by economic differences (Steiner 1971, 15–44).

By contrast how would one go about loosening the grip of France's equally ancient centralized bureaucracy in order to introduce a dose of local autonomy healthy for attaining reformist or transformative goals? That system, long in the making, has remained virtually unchanged through monarchies, empires, and republics. As Deschanel put it for the Fourth Republic, "The republic on top and the empire underneath" (Williams 1954, 2). The system is a prototypical example of cultural "inertia." In less-developed nations the difficulty of balancing centralization and decentralization tends to be greatly compounded by the fact that central permeation of local areas generally is highly imperfect. Many developing nations, it has been said, are "heads without bodies." An effective central administration that can effectively control and supervise at the local level seems itself to require prolonged development. This includes the development of suitable cultural dispositions, as Weber argued in his justly influential work on bureaucracy. It also requires modification of profound cultural (tribal or communal) identities. Resources matter, too, but they are probably the least obstacle.

Take another example. Werlin argues that one of the "software tools" by which politics may overcome culture is to obtain subordinate participation in project decisions (though, somehow, without lessening the capacity of leaders). This will legitimize actions and goals and, presumably, provide information to leaders of "where the shoe pinches." But how are obstacles to sufficient participation (discussed in a great many studies and generally rooted in cultures or subcultures) to be overcome? How is the "tool" of participation to be fashioned? Werlin does not address this, as if it were a simple matter of "project design" to get people to participate.

But inducing participation has been one of the great, unresolved problems of democratic theory and practice. It is difficult to deny Almond and Verba's evidence that there are more or less "participant cultures" (1963) or Verba and Nie's finding that even in a highly participant culture, the United States, only about 10% participate much in politics except to vote (1972). John Stuart Mill sought the remedy in the high devolution of power to local governments (1910, 347–48), which are more proximate to citizens and in which, consequently, they might participate frequently and regularly. Pateman (1970) puts more emphasis on still more proximate participation in the workplace. It should be noted that in both cases, the point is to form something cultural—general participatory dispositions. It should also be noted that the formation of such dispositions, even in institutions as proximate and important to people as workplaces, has proved extremely difficult, not least because of cultural factors. On this, I recently wrote, citing evidence, that "not all the piety and wit of workplace reformers have been able to dent the inertia of the existing structure of authority" (Eckstein 1984, 126). For a superior, I think generalizable, case study of the difficulties, even under what seem ideal circumstances, I refer readers to an important book by John F. Witte, *Democracy, Authority, and Alienation at Work* (1980). He finds, in essence, that workers do not participate much even with as much encouragement as is conceivable and that those who do participate are highly atypical—either workers who participate to advance into (be coopted by) the hierarchy or rebels who use participatory opportunities as platforms to make trouble. (So much for legitimacy.)

One could go through the rest of the "tools" listed similarly. In fact, some make easier prey for culturalists, for instance, assuring the independence of certain officials, getting leaders (Khomeini? Noriega?) to use gentle influence and persuasion, avoiding ethnic conflict, or getting consensus on objectives. But space forbids. In any case, the point should be sufficiently made. The software and hardware by which, according to Werlin, politics may overcome culture only leads us back to the institutions and relationships that are themselves essential elements of political cultures. That is not to say that with proper understanding and in proper ways, they cannot be used at all to bring about intended effects. It does mean that Werlin's so-called solutions to a large extent only describe the *problems* of political engineering.

El Salvador is hardly a good example of "soft power," Egypt of a balance of decentralized power and central control, nor India of the absence of dissensus or discrimination, working large-scale changes. But the stability of contemporary West German democracy is invoked, as it was by Rogowski (1974), as a glaring (though singular) case of nonrevolutionary transformation to invalidate culturalist theories of political change. I want to dwell on that case a bit more than in ny original article, because the West German case, properly understood, seems to me, precisely because of its singularity, to score points *for* culturalist theory.

I pointed out that West Germany has often been considered a "deviant case," in the sense of an exception to be disregarded, but I also stated that just treating it as exceptional is improper. The utility of deviant cases is that they can tell us a great deal about typical cases, precisely through their singular nature. An exemplary use of deviant case analysis for this purpose is the Lipset, Trow, and Coleman (1956) study of democracy in the International Typographical Union (ITU) (at any rate as it was). The study is relevant here simply because West Germany may reasonably be considered the ITU of political systems—at least, up to a certain chronological point, since it is distinctly possible, and quite consistent with my thesis, that over the last half century German democracy has become increasingly "enculturated."

The ITU was a "deviant" case among trade unions because it had a highly democratic structure where oligarchy generally prevails. It had a genuine two-party system; it used referenda copiously (often to overturn leaders' preferences—not least leaders' preferences for salary increases); it held regular, well-attended meetings of the members; it had an institutionalized (and varying) opposition able to print and distribute material, including a newspaper; and so on. If one could understand both what conditions made for this and that the conditions are highly exceptional, even unique, among unions (and perhaps other organizations) one could have, at the same, an explanation of the ITU's structure and *confirmation* of the iron law of oligarchy. Without going into details, this is exactly what emerges from the Lipset, Trow, and Coleman (1956) study and may emerge from any study that follows Kendall and Wolf's notion that deviant cases not anticipated by theory, can, in many cases, be the cases most fruitful for theory building, provided they are not used simply to "tidy up" loose ends by giving some plausibility to exceptions and thus disposing of them (Kendall and Wolf 1949, 12–13). "Deviant case analysis," in the technical sense, both explains cases and *validates* the general rules from which they differ.

What makes Germany such a case, I argued sometime ago (Eckstein 1979), is that Germany has, ever since unification, undergone shock wave after shock wave of generally disorienting experiences, far more so than any other country: not just Nazism and the war, but rapid industrialization in the nineteenth century, the first World War and the loss of much of a whole male generation, rapid and extreme democratization in the Weimar Republic, the Great Inflation and the Great Depression, and, after the traumas of Nazism and the Second World War, the occupation, the great influx of refugees from East Germany, and the stratificatory dislocations attendant on the *Wirtschaftswunder*. The point is that Germany is a case in which culture, due to large-scale disorientation after disorientation should be regarded as extremely weak and thus as unable to provide much friction against either raw political power (the Nazis) or reconstruction through rationally devised law (the Federal Republic). (The rationale that leads us to this point is provided in detail in Eckstein 1979 [p. 14].)

It would be unfortunate if my article were taken as an attempt to demonstrate the futility of any and all "political engineering." But agencies like USAID and the World Bank, and, more generally, constitution makers, nation builders, and, indeed, the many admirable people who have sought to "improve" others in, for instance, the foreign and domestic Peace Corps—in fact all of us—have much to learn about such engineering. A few sympathetic books free of comfortable illusions, like Foster's on inner-city teaching (1974), have reminded us of this. But naturally, we would rather believe that all can be done, and done rapidly, by using (preferably simple) correct techniques.

In regard to culture and political engineering, I want to make just two points, in conclusion. First (to repeat a critical point), as engineers must work with material givens, so political engineers must work with human—that is, cultural—givens, not least if their aim is to modify them. This point, at present, emerges especially clearly in retrospectives on our ineffective-to-disastrous counterinsurgency policies (an outstanding example is Shafer 1988). Secondly, the less strongly entrenched cultures are, the more susceptible they should be to political engineering, the Federal Republic being an extreme case in point. Perhaps that makes peasant cultures disoriented by the intrusion of capitalist market forces relatively easy to direct toward collective violence, as has been argued (see, among others, Wolf 1969). Perhaps, as well, the relative weakness and flexibility of culture of highly developed, "rationalized" societies makes them relatively hospitable milieus for politically engineered social change, compared to less-developed nations.

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The following articles, research notes, and controversies, have been tentatively scheduled for publication in the June 1990 issue:

Frances Stokes Berry and William D. Berry. "State Lottery Adoptions as Policy Innovations: An Event History Analysis."

William Bianco, Peter Ordeshook, and George Tsebelis. "Crime and Punishment: Are One-Shot, Two-Person Games Enough?" A Controversy.

Lawrence Bobo and Frank Gilliam, Jr. "Race, Sociopolitical Participation, and Black Empowerment."

James E. Campbell and Joe A. Sumners. "Presidential Coattails in Senate Elections." A Research Note.

Charles M. Cameron, Albert D. Cover, and Jeffrey A. Segal. "The Puzzle of Roll Call Voting on Supreme Court Nominees." A Research Note.

James Farr, John Gunnell, Raymond Seidelman, John Dryzek, and Stephen Leonard. "Can Political Science History Be Neutral?" A Controversy.

Norman Frohlich and Joe A. Oppenheimer. "Experiments on Taxation, Participation, Stability, and Productivity."

Richard L. Hall and Frank Wayman. "Buying Time: Moneyed Interests and the Mobilization of Bias in Congressional Committees."

Jon A. Krosnick and Donald R. Kinder. "Altering the Foundations of Popular

Political Culture and Change

Support for the President through Priming: Reagan and the Iran-Contra Affair." A Research Note

Arend Lijphart. "The Political Consequences of Electoral Laws, 1945-85." A Research Note.

Paula D. McClain and Albert K. Karnig. "Black and Hispanic Socioeconomic and Political Competition." A Research Note.

Roslyn Simowitz and Barry L. Price. "The Case of the Expected Utility Theory of Conflict: Measuring Theoretical Progress."

Jack Tweedie, Dennis D. Riley, John Chubb, and Terry Moe. "Should Market Forces Control Educational Decision Making?" A Controversy.

John R. Wright. "Contributions, Lobbying, and Committee Voting in the U.S. House of Representatives."

BOOK REVIEWS

BOOK REVIEW CONTENTS

SCIENCE, RIGHTS, AND CONTROL OF THE BODY

Dutton, Diana B., Thomas A. Preston, and Nancy E. Pfund. *Worse Than the Disease: Pitfalls of Medical Progress*
  Grobstein, Clifford. *Science and the Unborn: Choosing Human Futures*
  Mathieu, Deborah, ed. *Organ Substitution Technology: Ethical, Legal, and Public Policy Issues* . . . . Barbara M. Rowland 269
Eisenstein, Zillah R. *The Female Body and the Law* . . . . Nancy J. Hirschmann 271
Pateman, Carole. *The Sexual Contract* . . . . Sharon N. Snowiss 272
Blank, Robert H. *Life, Death, and Public Policy* . . . . Lynton K. Caldwell 274

POLITICAL THEORY

Arac, Jonathan, ed. *After Foucault: Humanistic Knowledge, Postmodern Challenges* . . . . Shane Phelan 275
Bumiller, Kristin. *The Civil Rights Society: The Social Construction of Victims* . . . . Harold H. Walton 277
Callinicos, Alex. *Making History: Agency, Structure, and Change in Social Theory* . . . . Glenn N. Schram 278
Etzioni, Amitai. *The Moral Dimension: Toward a New Economics* . . . . Aaron Wildavsky 279
Gellner, Ernest. *Plough, Sword, and Book: The Structure of Human History* . . . . Robert Booth Fowler 280
Inglis, Fred. *Popular Culture and Political Power*
  White, John Kenneth. *The New Politics of Old Values* . . . . Arthur Asa Berger 280
Kammen, Michael. *Sovereignty and Liberty: Constitutional Discourse in American Culture* . . . . Michael W. McCann 282
McNally, David. *Political Economy and the Rise of Capitalism: A Reinterpretation* . . . . Conrad P. Waligorski 284
Nielsen, Kai. *Marxism and the Moral Point of View: Morality, Ideology, and Historical Materialism* . . . . Oskar Gruenwald 285
Rosenberg, Shawn W. *Reason, Ideology, and Politics*
  Rosenberg, Shawn W., Dana Ward, and Stephen Chilton. *Political Reasoning and Cognition: A Piagetian View* . . . . Richard Ashcraft 286
Sica, Alan. *Weber, Irrationality, and Social Order* . . . . Ira J. Cohen 288

AMERICAN POLITICS

Adams, Carolyn Teich. *The Politics of Capital Investment: The Case of Philadelphia*
  Monkkonen, Eric H. *America Becomes Urban: The Development of U.S. Cities and Towns, 1780–1980*
  Smith, Michael Peter. *City, State, and Market: The Political Economy of Urban Society* . . . . Susan E. Clarke 290
Brown, Thomas. *JFK: History of an Image*
  Snyder, J. Richard, ed. *John F. Kennedy: Person, Policy, Presidency* . . . . Larry Berman 292

AMERICAN POLITICAL SCIENCE REVIEW
VOLUME 84 NO. 1 MARCH 1990

- Bruce, Steve. *The Rise and Fall of the New Christian Right: Conservative Protestant Politics in America 1978–1988* . . . . . J. David Woodard 292
- Cohen, Jeffrey E. *The Politics of the U.S. Cabinet: Representation in the Executive Branch, 1789–1984* . . . . . Kevin V. Mulcahy 293
- Cole, Leonard A. *Clouds of Secrecy: The Army's Germ Warfare Tests over Populated Areas* . . . . . Frances V. Harbour 294
- Cortner, Richard C. *A Mob Intent on Death: The NAACP and the Arkansas Riot Cases* . . . . . Lee Epstein 296
- Curry, Richard O., ed. *Freedom at Risk: Secrecy, Censorship, and Repression in the 1980s*
  - Friedelbaum, Stanley H., ed. *Human Rights in the States: New Directions in Constitutional Policymaking* . . . . . Kim Lane Scheppele 297
- Denton, Robert E., Jr. *The Primetime Presidency of Ronald Reagan: The Era of the Television Presidency*
  - Jamieson, Kathleen Hall and David S. Birdsell. *Presidential Debates: The Challenge of Creating an Informed Electorate* . . . . . Stanley Kelley, Jr. 298
- Erie, Steven P. *Rainbow's End: Irish-Americans and the Dilemmas of Urban Machine Politics, 1840–1985* . . . . . Thomas A. Flinn 300
- Estreicher, Samuel, and John Sexton. *Redefining the Supreme Court's Role: A Theory of Managing the Federal Judicial Process* . . . . . Judith A. Baer 301
- Foreman, Christopher H., Jr. *Signals From the Hill: Congressional Oversight and the Challenge of Social Regulation* . . . . . Mathew D. McCubbins 302
- Godwin, R. Kenneth. *One Billion Dollars of Influence: The Direct Marketing of Politics*
  - Stern, Philip M. *The Best Congress Money Can Buy* . . . . . John P. Frendreis 304
- Goodsell, Charles T. *The Social Meaning of Civic Space: Studying Political Authority through Architecture* . . . . . Joseph F. Freeman 305
- Grantham, Dewey W. *The Life and Death of the Solid South*
  - Lamis, Alexander. *The Two-Party South* . . . . . Clifton McCleskey 306
- Hargrove, Erwin C. *Jimmy Carter As President: Leadership and the Politics of the Public Good*
  - Spencer, Donald S. *The Carter Implosion: Jimmy Carter and the Amateur Style of Diplomacy*
  - Rozell, Mark J. *The Press and the Carter Presidency* . . . . . Barbara Hinckley 307
- Herrnson, Paul S. *Party Campaigning in the 1980s* . . . . . Kay Lawson 309
- Horwitz, Robert Britt. *The Irony of Regulatory Reform: The Deregulation of American Telecommunications* . . . . . John E. Rouse, Jr. 310
- Koven, Steven G. *Ideological Budgeting: The Influence of Political Philosophy on Public Policy* . . . . . Marcia Lynn Whicker 311
- Lasser, William. *The Limits of Judicial Power: The Supreme Court in American Politics* . . . . . Gary L. McDowell 312
- Lea, James F., ed. *Contemporary Southern Politics* . . . . . Robert P. Steed 313
- Loss, Richard, ed. *Corwin on the Constitution.* Vol. 3, *On Liberty against Government* . . . . . Charles W. McCurdy 314
- Lutz, Donald S. *The Origins of American Constitutionalism*
  - Jillson, Calvin C. *Constitution Making: Conflict and Consensus in the Federal Convention of 1787* . . . . . Glenn A. Phelps 315
- Mezey, Susan Gluck. *No Longer Disabled: The Federal Courts and the Politics of Social Security Disability* . . . . . Deborah A. Stone 316

Book Reviews

Pffiffner, James P. *The Strategic Presidency: Hitting the Ground Running* . . . . Francis E. Rourke 318
Pomper, Gerald M. *Voters, Elections, and Parties: The Practice of Democratic Theory* . . . . Cornelius P. Cotter 319
Prindle, David F. *The Politics of Glamour: Ideology and Democracy in the Screen Actors Guild* . . . . Thomas J. Keil 320
Regens, James L., and Robert W. Rycroft. *The Acid Rain Controversy* . . . . Regina Axelrod 322
Robinson, Donald L. *"To the Best of My Ability": The Presidency and the Constitution* . . . . Richard M. Pious 322
Sheldon, Charles H. *A Century of Judging: A Political History of the Washington Supreme Court* . . . . Herbert Jacob 324
Weaver, R. Kent. *Automatic Government: The Politics of Indexation* . . . . Jack H. Knott 325
Welch, Susan, and Timothy Bledsoe. *Urban Reform and Its Consequences: A Study in Representation* . . . . Thomas R. Dye 326
Welfeld, Irving. *Where We Live: A Social History of American Housing* . . . . Peter Marcuse 327
Winant, Howard A. *Stalemate: Political Economic Origins of Supply-Side Policy* . . . . Mark Rom 328

COMPARATIVE POLITICS

Bilocerkowycz, Jaroslaw. *Soviet Ukrainian Dissent: A Study of Political Alienation* . . . . Yaroslav Bilinsky 330
Brand, Laurie A. *Palestinians in the Arab World: Institution Building and the Search for State* . . . . Rex Brynen 331
Brustein, William. *The Social Origins of Political Regionalism: France, 1849–1981* . . . . Karen J. Vogel 332
Decalo, Samuel. *Psychoses of Power: African Personal Dictatorships* . . . . Jeanne-Marie Col 333
Desjardins, Robert. *The Soviet Union through French Eyes, 1945–1985* . . . . Joan Barth Urban 334
Gilberg, Trond, ed. *Coalition Strategies of Marxist Parties* . . . . Stephen J. Blank 336
Grindle, Merilee S. *Searching for Rural Development: Labor Migration and Employment in Mexico* . . . . Richard C. Jones 337
James, Daniel. *Resistance and Integration: Peronism and the Argentine Working Class, 1946–1976* . . . . Alberto Ciria 338
Joffe, Ellis. *The Chinese Army after Mao* . . . . Jonathan D. Pollack 339
Lewis-Beck, Michael S. *Economics and Elections: The Major Western Democracies* . . . . James E. Alt 340
Lieberthal, Kenneth, and Michel Oksenberg. *Policy Making in China: Leaders, Structures, and Processes*
Ross, Lester. *Environmental Policy in China* . . . . Susan L. Shirk 341
McIntyre, Angus, ed. *Aging and Political Leadership* . . . . James N. Schubert 343
Malloy, James M., and Eduardo Gamarra. *Revolution and Reaction: Bolivia, 1964–1985* . . . . Waltraud Queiser Morales 344
Markovits, Andrei S., and Mark Silverstein, eds. *Politics of Scandal: Power and Process in Liberal Democracies* . . . . Paul E. Peterson 346
Panebianco, Angelo. *Political Parties: Organization and Power* . . . . Joseph LaPalombara 347
Sykes, Patricia Lee. *Losing from the Inside: The Cost of Conflict in the British Social Democratic Party* . . . . Jorgen Rasmussen 348

Uglow, Steve. *Policing Liberal Society*
Brewer, John, Adrian Guelke, Ian Hume, Edward Moxon-Browne, and Rick Wilford. *The Police, Public Order, and the State: Policing in Great Britain, Northern Ireland, the Irish Republic, the USA, Israel, South Africa, and China*
Marx, Gary T. *Undercover: Police Surveillance in America* . . . . Athan Theoharis 349
Whyte, William Foote, and Kathleen King Whyte. *Making Mondragon: The Growth and Dynamics of the Worker Cooperative Complex* . . . . Kenneth R. Hoover 351
Wiarda, Howard J. *The Transition to Democracy in Spain and Portugal* . . . . Richard Gunther 352

INTERNATIONAL RELATIONS

Clapham, Christopher. *Transformation and Continuity in Revolutionary Ethiopia*
Harbeson, John W. *The Ethiopian Transformation: The Quest for the Post-Imperial State*
Keller, Edmond J. *Revolutionary Ethiopia: From Empire to People's Republic* . . . . Terrence Lyons 354
Evangelista, Matthew. *Innovation and the Arms Race: How the United States and the Soviet Union Develop New Military Technologies* . . . . Glenn Palmer 355
Feinberg, Richard E., and Ricardo Ffrench-Davis, eds. *Development and External Debt in Latin America: Bases for a New Consensus*
Kuczynski, Pedro-Pablo. *Latin American Debt* . . . . Miles Kahler 356
Golan, Galia. *The Soviet Union and National Liberation Movements in the Third World*
Shultz, Richard H., Jr. *The Soviet Union and Revolutionary Warfare: Principles, Practices and Regional Comparisons* . . . . W. Raymond Duncan 358
Harding, Harry. *China and Northeast Asia: The Political Dimension* . . . . Steve Chan 360
Ikenberry, G. John. *Reasons of State: Oil Politics and the Capacities of American Government* . . . . Jonathan D. Aronson 361
James, Patrick. *Crisis and War* . . . . Douglas P. Lackey 362
Kam, Ephraim. *Surprise Attack: The Victim's Perspective* . . . . Michael Barnett 363
Morley, Morris H. *Imperial State and Revolution: The United States and Cuba, 1952–1986*
Brenner, Philip. *From Confrontation to Negotiation: U.S. Relations with Cuba* . . . . Louis A. Pérez, Jr. 364
Newsom, David D. *Diplomacy and the American Democracy* . . . . Robert Mandel 366
Parenti, Michael. *The Sword and the Dollar: Imperialism, Revolution, and the Arms Race* . . . . David T. Jervis 367
Ross, Robert S. *The Indochina Tangle: China's Vietnam Policy, 1975–1979* . . . . William S. Turley 368
Scott, Harriet Fast, and William F. Scott. *Soviet Military Doctrine: Continuity, Formulation, and Dissemination*
Catudal, Honoré M. *Soviet Nuclear Strategy from Stalin to Gorbachev: A Revolution in Soviet Military and Political Thinking* . . . . William C. Wohlforth 397

Book Reviews

Sigal, Leon V. *Fighting to a Finish: The Politics of War Termination in the United States and Japan, 1945* .... Steve Weber 370
Wooley, Wesley T. *Alternatives to Anarchy: American Supranationalism since World War II* .... Roger A. Coate 372

SCIENCE, RIGHTS, AND CONTROL OF THE BODY

Worse Than the Disease: Pitfalls of Medical Progress. By Diana B. Dutton, Thomas A. Preston, and Nancy E. Pfund. New York: Cambridge University Press, 1988. 520p. $29.95.

Science and the Unborn: Choosing Human Futures. By Clifford Grobstein. New York: Basic Books, 1988. 208p. $18.95.

Organ Substitution Technology: Ethical, Legal, and Public Policy Issues. Edited by Deborah Mathieu. Boulder: Westview, 1988. 340p. $59.00.

These three books illustrate the central issues and dilemmas of biomedical policy. They also reveal the unevenness and complexity of the public policy literature addressing biomedicine. This ever-growing literature is multidisciplinary and interdisciplinary. Basic questions of political philosophy, such as justice, fairness, and freedom are juxtaposed with debates over the role of science and technology and on-going theoretical controversies about the formation and implementation of public policy.

In *Science and the Unborn*, Clifford Grobstein's central goal is to examine the stages of development of the unborn in an effort to help formulate public policy regarding the unborn. Much of the book focuses on the appropriate terminology for various stages of the unborn (e.g., preembryo, embryo, fetus), and what "status" of membership in the human community should accompany each stage of development. The discussion of our rapidly increasing knowledge about the life of the unborn from fertilization to birth is clear and readable. Grobstein addresses many of the difficult questions posed by in vitro fertilization and the increasing use of technology to assess the health of the unborn. He is sensitive to the issue of "reproductive privacy"; and he raises an oft-neglected question in the abortion debate, namely, "whether and how much society is willing to help in meeting the burdens of extraordinary care" posed by severely handicapped children. Overall, then, Grobstein considers and urges the reader to consider deeply troubling issues in the field of biomedicine as it relates to the unborn. Those interested in that area will find the book informative.

As a contribution to the broader policy debates around biomedicine, *Science and the Unborn*'s value is limited by the overwhelming priority Grobstein places on the pursuit of scientific knowledge. One of the difficulties in the book is that Grobstein has an agenda other than mere clarification of terms in order to advance the policy debate on the unborn. While he mentions the need for a moral consensus in order to formulate public policy on the unborn, his primary goal emerges as the much more comprehensive goal of the pursuit and advance of science. Thus, whatever agreements we reach about policies toward the unborn, those agreements "ought not unduly to impede increase of knowledge and further human evolutionary progression" (p. 136). The clear implication that restricting scientific research on the unborn will somehow impede the evolution of the human species reveals how unquestioningly the goals of a presumably value-neutral science are accepted. More troubling still is the comment by Grobstein that preembryos, if they cannot realize their potential as persons, can be put "to use in ways that are fully reflective of their value as members of the human community" (p. 93). "Using" preembryos carries with it the judgment either that preembryos are *not* human, or that "using" persons is acceptable to advance scientific research. His desire for unrestricted research emerges most clearly in his recommendation of a decision-making process for defining the status of the unborn modeled on the recombinant DNA controversy, a process that resulted in molecular genetic research "free of limitation."

Others, such as Diana B. Dutton and her colleagues in *Worse Than the Disease: Pitfalls of Medical Progress*, see the DNA decision-making process as an instance in which scientific elites succeeded in avoiding public debate and accountability. As the title suggests, this volume questions the confidence often placed in medical progress and technology by scientists and physicians. Dutton's work fits within the genre of biomedical policy literature that focuses on the need for greater public authority and participation, with less authority granted to technical and scientific experts. This focus results in a call for restructuring medical policy making alongside the perspective that modern medicine ought to emphasize preventive medicine rather than curative medicine. That Dutton follows this line of thought predictably, with little critical assessment of it, is disappointing.

The case studies themselves, on DES (diethylstilbestrol, a synthetic estrogen), the swine flu immunization program, the artificial heart, and genetic engineering, are well researched and extremely useful for a variety of purposes. These case studies document a consistently unwarranted optimism about the state of the art of medical intervention and scientific knowledge. Dutton emphasizes the dominance of insulated elites, medical and other, and devotes little time to the supporting cultural ethos valuing "mastery" over nature. The case of DES raises particularly chilling concerns about the nature of physician decisions to prescribe and use drugs, the marginal role of the FDA in protecting anyone, and the treatment of women by the medical profession. For example, Dutton recounts one physician's experiences, where initial testing of DES on dogs was not encouraging, as the dogs died. The scientist (at the suggestion of E. R. Squibb and Sons, a DES manufacturer) then tried it on women. DES worked "better" on women. It didn't kill them and appeared to stop uterine bleeding. So "a rule was made . . . that all women coming to the Emergency Room, Gynecological or Menstrual Disorder Clinic were to receive an injection" (p. 48). This was not, unfortunately, an isolated case. Physicians routinely disregarded the known carcinogenic effects of DES on animals. Numerous scientists, manufacturers, the FDA, and practicing physicians chose to proceed with experiments on women's bodies and unborn children without informing individuals of the risks they faced.

Drawing upon the case studies, Dutton raises key questions about biomedical policy making: Are technology and progress indistinguishable? Are they good? and What are the limits on society's responsibility for the health care of its members? *Worse Than the Disease* also addresses issues of decision making. The swine flu case aptly illustrates the well-known pitfalls of "closed" decision making. Students and public administrators alike would benefit from acquaintance with it, as well as with Dutton's prescription for better decision making. That prescription is succinct and consistent with other recent studies on decision making. Regarding the limited successes of citizens and dissenting scientists in shaping public policy in these four cases, Dutton advocates "the large-scale mobilization of an informed and determined citizenry" (p. 374) to influence biomedical policy. She only briefly addresses the many obstacles in the way of such a mobilization.

Deborah Mathieu's edited book, *Organ Substitution Technology*, draws from the work of well-known scholars representing a number of academic disciplines. The book is intended to advance debate on the difficult issues of organ donation, distribution, and transplantation by looking at a host of relevant issues and offering some proposals. Overall, despite some unevenness of contributions, the book succeeds as a reference tool for those interested in these issues. Major sections are devoted to donation and distribution, cost issues, and an extended case study of the artificial heart. The essays by Mathieu are a heroic effort to pull the pieces together, although the absence of some concluding comment detracts from the whole. This book emphasizes how organ transplantation policy raises the ethical questions of biomedicine in sharp outline. Most difficult among these questions are, How do we decide whose lives to save? How are the staggering costs of transplants to be funded? How do we make these decisions fairly and equitably?

Mathieu's contributions are notable for their clarity and breadth. While calling for a reasoned, informed debate, and paying due respect to the need for cost-effectiveness analysis, Mathieu concludes that "there is no way of getting around the need for ethical theorizing in policy evaluation" (p. 17). Her discussion of fairness in allocation of organ transplantation is well grounded in the reality that "resources are finite and health is not the only good" (p. 145). She is forthright about the vagueness of the standard of providing a "decent minimum" of health care to all. In her introductory section on the artificial heart, she brings up the need for physicians to discuss with patients (in order for them to give truly "informed consent") the "possible acceptability of death over the chances of life" (p. 240). Her comments alone serve as a valuable and concise introduction to the dilemmas of organ transplantation.

Arthur L. Caplan, Dan Brock, George J. Annas, H. Tristam Engelhardt, and Dale Jamieson —along with Mathieu—place this volume in the company of other recent books focusing on the inevitability of the limits posed by the fact that—as Engelhardt puts it—we are "finite men and women who have neither the financial nor moral resources of gods and goddesses" (p. 229). Of the three books under review, *Organ Substitution Technology* is the most useful addition to the study of biomedical policy. Like other works, such as H. J. Aaron and W. B. Schwartz's *Painful Prescription: Rationing Hospital Care* (1984), D. Callahan's *Setting Limits: Medical Goals in an Aging Society* (1987), and G. Calabresi and P. Bobbit's classic *Tragic Choices* (1978), it advances our understanding of the difficult choices we must make in biomedical policy. Together, these books serve as a reminder that the approaches toward biomedical ethics and policy are diverse and that the policy issues posed by the advances of medical science have no simple solutions.

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The Female Body and the Law. By Zillah R. Eisenstein. Berkeley: University of California Press, 1988. 280p. $25.00.

In a period of affirmative action backpeddling and antiabortion violence, this timely examination of the problematic relationship of sexual difference and legal equality is warmly welcomed. The author's central thesis is that "because law is engendered, that is, *structured through the multiple oppositional layerings embedded in the dualism of man/woman,* it is not able to move beyond the male referent as the standard for sex equality" (pp. 42–43, emphasis original). By engaging in an argument for sexual equality that begins with the female body, Eisenstein not only offers insightful analyses of existing inequalities and possible solutions but also demonstrates how feminism is more theoretically powerful than other egalitarian frameworks. As in her earlier works, Eisenstein's method weaves together theory and political "practices" to make her case. Beginning from a Foucauldian perspective on difference and power, she analyzes court cases and executive policies that illustrate how the rubric of equality is used to treat women unequally.

Eisenstein challenges the concept of equality as "sameness," because for women this generally means "the same as white men." Taking (white) men as the standard catches women in a double bind: either women are treated exactly the same as men, thus denying their specific needs (as in *California Federal* v. *Guerra),* or else they are treated as completely different from men and in need of special protection *(Equal Employment Opportunity Commission* v. *Sears).* Both of these approaches to difference and equality are inherently masculinist because they pass off male experience as "gender-neutral." This is not only self-contradictory, Eisenstein maintains, but discriminates against women at the very core of our legal system: women cannot be equal to men *as women.* Taking men as the standard imparts—at the same time that it denies—the cultural meaning of gender that we then ascribe to "nature" or sex.

In order to "decenter the phallus," she suggests that we take the pregnant female body as an alternative starting point for assessing equality. Taking pregnancy as a new standard does more than remind us of the hegemony of male imagery in theory, law, and politics throughout history. It also requires us to incorporate a notion of diversity into our concept of equality by emphasizing how humans, as embodied beings, are "dynamic, in the process of change, rather than . . . static or essentialist" (p. 93). Pregnancy thus "undermines any *one* preferred standard" (p. 2), at the least because men cannot become pregnant.

Hence, it is not just whether and how men and women differ that is at issue in assessing equality but also what it means to say that we "differ." Eisenstein argues that within liberalism, it means a series of hierarchical dualities: culture over nature, mind over body, male over female. She seeks to replace this with a radical pluralism that can account for specific differences without either reducing difference to its gendered form or producing outcomes of inequality. Yet at the same time Eisenstein is critical of those who take difference too far. To deny the shared experiences of pregnancy, abortion, rape, and sexual harrassment disempowers women. "We need . . . a method that focuses on the relation between similarity and difference" (p. 18), she argues: "The challenge is to specify the unity and not to deny it" (p. 40). Beginning from the pregnant female body enables us to particularize group and individual experiences and hence to develop gender-specific legislation that does not "engender" inequality (p. 206). It also requires us to recognize that issues such as abortion, parental leave, childcare and infant care—all currently marginalized as "just women's issues"—are in fact issues for all of us and that confronting them is a necessity, not a luxury (p. 191).

However, in "Reconstituting the Phallus" (chaps. 4-5), the book does not fully realize its exciting opening claim that engendered inequality is an epistemological problem. Rather than showing how and why the problems of sex equality relate to liberal epistemology, Eisenstein shows how they stem from neoconservative values and a sexist political climate. While this is an important insight, there is insufficient argumentation that dominant legal discourses are *premised* on women's exclusion. At the same time, while white men hold most of the power and do most of the oppressing in Western cultures, it is not necessarily whiteness or maleness per se that causes "engendered inequality." Even white males differ from one another. They may be poor, paralyzed, divorced, or depressed and may suffer oppression as a result. Rather, the social construction of maleness—which has come to define humanity—is at issue. The question then arises how (even) white males got to be so dehumanized by this static (mis)representation in liberal and legal discourses. Again, the answer is epistemological.

The uncompleted epistemological theme in turn makes taking pregnancy as a standard somewhat problematic. Eisenstein says she does not seek to replace one hegemony with another. But she does not explain how to avoid this except by repeatedly invoking difference. Many feminist postmodernists reject the notion of a "standard" altogether. Others, suspicious of biological imagery, will take umbrage at the particular standard she chooses. A fuller discussion of current feminist debates over postmodernism could anticipate such objections and give greater substance to "difference" as a political concept.

Some readers will conclude that the book finally fails to decide what it is. Neither theory, nor U.S. politics, nor public law, it needs "more" of each of these. While I would indeed like to see more of Eisenstein's very persuasive arguments, her transcendance of categories is a strength; for it allows her to introduce cross-disciplinary methods and questions often necessary to feminist inquiry. Whatever the reader's disposition, the book raises significant and controversial issues in several fields. Its efforts to carve out a middle ground between a poststructuralist extremism and a unified feminist politics provide an important contribution to the growing field of feminist political theory concerned with the subtle and extremely complex problem of difference.

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The Sexual Contract. By Carole Pateman. Stanford, CA: Stanford University Press, 1988. 264p. $39.00 cloth, $12.95 paper.

In a dual movement, Pateman provides a rereading of classic political texts on the origin of the state to include the often-forgotten but essential a priori sexual contract and simultaneously uncovers the patriarchical categories that feminists have unwittingly used in discussions of patriarchy itself. In a carefully documented argument, Pateman shows that in the conjectural histories of societal origins from Hobbes to Freud the first contract (by force or exchange of obedience for protection) is the domination and subordination of women by men. Only after that foundation can civil society emerge from a contract among men that establishes fraternal bonding among politically determined equals. Women are not party to this second social contract. The significance of this fact is that civil freedom results from contractual relations among equal individuals (the second contract) and therefore depends upon patriarchical right.

The concept of contract rests on the assumption that "individuals" possess their own bodies and freely engage in promises and exchanges in the political and economic realms. *Individual,* however, is synonymous with *male,* for only the male is viewed as possessing his body. Women are subordinate to male right in any contract situation from marriage to employment to surrogate motherhood. This subordination results from masculine sexual mastery "involving possession and access to sexual property" (p. 185). Hence, women do not fully and freely possess their own bodies and are always self-contradictory when speaking of the "individual" or of free "contract" to argue for "rights."

The public-private distinction and the fact of women's initial exclusion from the public realm is not new. Pateman's contribution, however, consists in the recognition of the displacements that obfuscate sexual dominance in the guise of equal rights and consent through contract. The categories and logic developed to explain political relationships are inherently masculine. The sexual difference is a political difference. Women lack the same civil standing as men despite gains made in the last century.

She argues that the idea of contract itself is patriarchical and its use as a concept by feminists shifts the discourse within a patriarchical paradigm: "The feminist dream is continuously subverted by entanglement with contract" (p. 188). She concludes that "a free social order cannot be a contractual order" (p. 232) and that "the sexual contract and the social contract, the 'individual' and the state, stand and fall together" (p. 232).

The scope and sweep of the work is ambitious. In focusing on the sexual contract as the most basic relationship, underlying all other structures in modern society, she risks using the concept metaphorically and superficially. Her examples are carefully chosen to illustrate explicit contract language—marriage, employment, prostitution, surrogate motherhood—but leaves out discussions of other values, rights, limits that the society is struggling with as it attempts to resolve these relationships. Reduction of all relationships to this primitive sexual contract simplifies what are complex issues. The value of the simplification is that it starkly points out one critical perspective that needs to be examined in analyzing these relationships. But if used too extensively, it destroys experiential attempts to alter sexual relationships from this category of primal dominance. Can the first contract ever be dissolved?

Pateman's argument also rests on an assumption of sexual difference that must be maintained, respected, and valued in nonhierarchical terms. She attacks contract theory's seductive language that obscures the fact that "labor power," for instance, involves the subjection and use of women's bodies. She argues that to speak of "sex-neutral" bodies—individuals or gender—transforms women into men and represses the fact of fundamental loss and subjugation. Women's bodies are different in openness to nature ("permeable," changeable in shape, and "subject to cyclical processes" [p. 96]) and capable of giving birth. This embodied difference has been a basis for domination. How can it become a basis for equal respect? Is the importance of sexual difference a matter of political construction of artificial contract or something inherent in human nature that necessitates mastery and subordination?

Pateman seems to argue for the latter interpretation in her analysis of men's bodies—the stuff of individuals—"tightly enclosed within boundaries" and capable of political creation. She seems to agree with Irigaray's assessment of "homosexual" society when she notes that fraternity involves the necessity of heterosexual relations. If men contracted for the sexual use of one another's bodies, the basis of the social contract would be undermined. Male equality would devolve into competition for sexual mastery. Is this a particular historical view or an essential part of men's difference, in the form of an aggressive nature? Is coexistence, rather than dominance, possible? Pateman provides conceptual tools for perceiving and critiquing the dominance but not for emerging from it. The argument to preserve women's difference, then, is almost residual in order to avoid the "political defeat of women as women" (p. 187) without an articulation of what the importance of that preservation entails.

Pateman's reliance on this original sexual contract shapes her definition of patriarchy and makes it a crucial concept for her analysis.

She disagrees with other feminists who confuse the concept with paternal power and thereby deflect contractual relations between men and women. She develops a historical perspective that chronicles patriarchy's various transformations through its most recent variant involving the reappearance of father right in new contractual form (surrogate motherhood, for example). While she illustrates the vital development of patriarchy—the movement, deceptions, and double entendre that maintain its dominance in modern society—she seems to despair of the ability of political theory to undermine its life supports. In the face of her analysis, she calls for "a new story about freedom" but leaves the reader wondering what such a story might entail and from what ground (relationship) it might grow.

Pateman's book is an exciting and valuable study that provides a carefully reasoned articulation of the sexual contract as a category for understanding the foundation and development of patriarchical relationships in modern society. It provides an effective tool in giving insights into and critical analysis of aspects of contemporary political debates involving women's bodies. It demands a rethinking of the nature of contract, consent, and freedom as it relates to such issues as prostitution and surrogate motherhood. The difficulty is that Pateman's point of departure, primal sexual domination, itself needs to be examined and perhaps unmasked as one of those stories that men tell men in this patriarchical world.

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Life, Death, and Public Policy. By Robert H. Blank. De Kalb: Northern Illinois University Press, 1988. 187p. $22.50 cloth, $8.50 paper.

The author's preface states that the book "describes the policy context of a broad array of biomedical innovations and analyzes the policy problems these advances are creating." The substance of the book is essentially policy-oriented political science. It does not attempt an in-depth analysis of any biomedical technology, nor does it explore in detail the ethical debate surrounding biomedical issues: "Instead it focuses on the broader social-policy dimensions raised by this biomedical revolution in which even the basic definitions of life and death are challenged" (p. ix).

The author undertakes this effort in six chapters, the first and last of which develop generalizing statements regarding the policy context of biomedicine and making the hard choices in policy decisions. Four intervening chapters examine policy problems with reference to human genetic and reproductive intervention, prenatal intervention, biomedical issues within the life cycle, and death-related issues in biomedicine. There is an appended bibliography and index.

Robert Blank is the leading scholar in the study of public policy and the biomedical sciences, having already produced six books and numerous articles on this subject. Relating closely to the present volume are the books *Rationing Medicine* (1988), and *Political Implications of Human Genetic Technology* (1981). Blank's work is distinctive in part because relatively few political scientists have ventured into the biomedical field. To do so requires, at least, developing a literary competence in disciplines largely alien to most social scientists.

In view of public and news media concern and pressures for governmental interventions, one would expect biopolitical problems to attract a growing number of political science researchers. This appears to be happening, assisted by the Association for Politics and the Life Sciences (affiliated with the American Political Science Association) and with a publication outlet provided by the journal *Politics and the Life Sciences.* Political scientists are beginning to claim some of the research territory previously forfeited to legalists and ethicists.

In 1987 Robert Blank and three other political scientists produced for the Institute for Advanced Study at Indiana University a report on *Biotechnology: Public Policy and the Social Sciences—Critical Needs in Teaching and Research,* subsequently condensed in the National Academy of Sciences monthly, *Issues in Science and Technology* (Spring 1988). This report was directed especially to political scientists, addressing the role of academic political science departments, problems of curricular revision, links with biologists and support functions of university administrators, private foundations and government. Robert Blank's published work has pushed a salient issue into the field of public policy for biotechnology, opening a way for other social scientists.

Another distinctive attribute of Robert Blank's published work is that it has dealt explicitly with specific policy issues, including genetic screening and euthanasia. Most of the writing on biopolicy is of a more general and philosophical nature. Of course earlier writings in this subdiscipline were primarily intended to call attention and arouse awareness. This was the intent of my article "Biopolitics: Science, Ethics and Public Policy" (Caldwell, 1964, *Yale Review)* and a recent book, *Biocracy: Public Policy and the Life Sciences* (1988). But it has been biologists rather than social scientists who have tried to direct public attention to the policy implications of advances in the life sciences and techno-economic threats to the biosphere. *Biology and Bureaucracy* (1986), edited by political scientists Elliott White and Joseph Lasco, represents "an effort to identify specific fields of political science where a life-science approach might be productive" (p. 43). This also is an exercise in consciousness-raising and illustrates a basic division in approaches to politics and the life-sciences.

The first approach is the examination of biological issues, concepts, and technologies as objects of public policy. This approach (i.e. biopolicy) is exemplified by the work of Blank and Caldwell with incidental contributions from other political scientists. The field of academic biopolicy publication is relatively small, outweighed by reports of governmental and intergovernmental agencies, for example the U.S. National Academy of Sciences, the Office of Technology Assessment, and the European Economic Communities. This is also an area of extensive publication in law and ethics (e.g. Wadlington, Waltz and Dworkin's *Cases and Materials in Law and Medicine* [1989] and Van Rensselaer Patteor's *Bioethics: Bridge to the Future* [1971]).

The other approach includes the larger output of biology-related research by political scientists. Its distinctive character is the application of biological concepts and methods to the study of political behavior. Insofar as this approach implies a biological basis for human behavior, it has some relationship to sociobiology, although the premises and methods in these two research areas are largely different. Sociobiological and ecological concepts, for example inclusive fitness, have been used by political scientists in theory development (e.g. Roger Masters).

In summary, *Life, Death, and Public Policy* is a carefully researched, well written, and concise analysis of an important set of issues. It reflects the extensive earlier work of the author and contributes to a field of inquiry hitherto neglected by political scientists.

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POLITICAL THEORY

After Foucault: Humanistic Knowledge, Postmodern Challenges. Edited by Jonathan Arac. New Brunswick, NJ: Rutgers University Press, 1989. 224p. $29.00 cloth, $14.00 paper.

This volume is the result of a 1985 conference on Foucault's impact on knowledge in the humanities—and a lively conference it must have been. The essays range from philosophy to political and feminist theory to literary criticism, and they encompass a wide variety of responses to Foucault's work. The contributors include Sheldon Wolin, Isaac Balbus, Jana Sawicki, Edward Said, and David Couzens Hoy; and Arac has done a generally fine job of locating them in relation to one another.

Though there is a theme to the work, it is not a simple one. Arac says in the introduction that "whether they would be tabulated as 'for' or 'against' Foucault, all of the authors show that their understanding has been altered by the challenges [Foucault's] work has posed" (p vii). However, some of the authors show that their understanding has not been altered at all by Foucault and indeed that they can or will make no sense of him. This is especially the case for the essays by Wolin and Balbus. Both authors demonstrate familiarity with Foucault's writings, yet both quote him out of context and condemn him for saying what he never says.

This phenomenon may be the most interesting feature of "postmodern" thought in the United States. Intelligent, conscientious, even brilliant thinkers continue to misread postmodern writers. The great contribution of Hoy's lucid article is his explanation of this. Hoy asks the question "whether Foucault should be read as a modern or a postmodern" (p. 13) and in answering gives us a clear, thoughtful discussion of what these terms mean. His conclusion (that Foucault is, indeed, a postmodern) is less interesting than his journey. Hoy suggests that we are witnessing a paradigm shift in philosophy and that this is the problem with understanding postmodern thinkers. A paradigm shift drives a wedge between the old and the new knowledges, with the new being incomprehensible to the old.

This is demonstrated in the Wolin and Balbus essays in several ways. In "On the Theory and Practice of Power" Wolin says that "Foucault's central preoccupation was with power" (p. 181), which he conceived as "essentially negative and preventive" (p. 182). This is not only in contradiction with Foucault's repeated statements of his project, it is belied by Wolin's later description of the power-knowledge relation. Wolin's fear that Foucault has destroyed the notions of the political, of theory, and of practice rests upon a modernist reading of his work.

Isaac Balbus shares this modernism and uses it to define the opposition he sees between Foucault and (psychoanalytic) feminism. His goal, he states, is "to persuade Foucauldians to take feminist psychoanalytic theory seriously." He asserts that "this is precisely what Foucauldians will not do"; but he cites no one, because nothing of the sort has been written. Balbus reads Foucault as the enemy of psychoanalysis and Marxism, though Foucault says that these "provide in a fairly consistent fashion, tools for local research" *(Power/Knowledge* [1980], p. 81). Balbus argues that Foucault has made a universal out of power-knowledge, an "unholy alliance" "almost as old as Western civilization itself" (p. 151); but he asserts this on the basis of remarks in *The Archaeology of Knowledge* (1972) that contravene his thesis.

Why is this? Balbus, like Wolin, is committed to the enterprise of global theory. Balbus labels himself neo-Hegelian, while Wolin tells us that "the preferred form of theorizing" is that of Plato, Hegel, and Marx (p. 199). To such theorists, Foucault's method is baffling in its specificity. Jana Sawicki's "Feminism and the Power of Foucauldian Discourse" helps to clarify this issue. Written in response to Balbus, she asks two questions. Because the point of Balbus's work is to suggest that Foucauldians should utilize feminist mothering theory, she asks, (1) "Would a Foucauldian reject mothering theory *tout court?"* and more generally, (2) "Are appeals to totality whether historical or social, or to a continuous subject of history, indispensable to feminist theory?" Her answer to both questions is *no*. In explaining her answer, she illuminates for us some of the difficulties attendant upon a modern reading of Foucault; so her essay complements Hoy's.

The other articles in this collection are less directly relevant to political theorists, but they are also interesting and helpful. In his critique of Stephen Toulmin's work, Paul Bove argues that "Foucault helps us see that Toulmin's fetishization of disciplined, humanistic reason not only prevents the intellectuals' examination of the real social and material interests at work in the struggles within disciplines . . . but also blocks all critical reflection upon the nature of intellectual agency" (p. 57). Thus, he draws for us connections between Foucault's critique of epistemology and his political histories and activities.

All in all, this is an exciting volume. Everyone will find something in here to enjoy and something to challenge. The essays are all well written, and together they go quite a way toward outlining the frontiers of debate over Foucault's work. They will stimulate readers to return to Foucault's texts and develop their understanding of his thought.

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Book Reviews: Political Theory

The Civil Rights Society: The Social Construction of Victims. By Kristin Bumiller. Baltimore: Johns Hopkins University Press, 1988. 176p. $19.95 paper.

This study of the efficacy of legislative and judicial interpretation of discrimination seeks to prove why the effort to end discrimination has, for the most part, failed. Bumiller contends that legal remediation has served to perpetuate instead of ameliorate the effects of discrimination (p. 3). In her view, legal remedies have contributed to the exception, renunciation, and misinterpretation of victims and thus to a perpetuation of their passivity and tacit consent to such treatment (p. 3). She attributes the foregoing condition to the post–Civil War legal model that has served to provide legislative and adjudicative content for the Equal Protection Clause (p. 6). Indeed, she believes that the stratagem that gave rise to the 1964 Civil Rights Act works similarly to limit, instead of extend, governmental protection of victims of discrimination (p. 7).

Bumiller tests the role of process theory and its critics in the development of the model of legal protection of victims, includes a discussion of the issue of group versus individual rights, and acknowledges the criticism of legal formalism by the school of legal realists (pp. 11–25). The author surveys the theories of Mannheim, Lukács, and Marcuse in an attempt to discern why elite definitions of distributive justice structure victims' responses to legal domination (pp. 30–32). She concludes that the suggestions of these authors are incomplete and turns to Michel Foucault's analysis of power and legal alternation and Ralf Dahrendorf's sociological theory as the methodological orientation of her study. Casual reference is made to the theories of Bruno Bettleheim and Niklas Luhmann (pp. 32–39). Bumiller's concern is to establish a conjunction between her methodological stratagem and the reports of the respondents who comprise the body of the case study (pp. 52–58). She critically illuminates the historical passage of the effort to end discrimination, largely to show that it contains the hidden shoals of isolation, self-denial, and misconstruction of victims of discrimination (pp. 72–77).

In the study's analysis of the survival ethic of victims Bumiller uses Rahel Varnhagen and Malcolm X as examples of those who adopted strategies of assimilation and exclusion in an attempt to retain personal identity (pp. 84–87). Since this response differs so greatly from that of most contemporary victims (pp. 88–95), Bumiller concludes that a concept of collective rights is an essential requirement of a viable ethic of survival. Indeed, the experience of injustice by most people will have no meaning except, in Arendt's words, "modern loss of respect" (pp. 95–97).

The study ends with an assessment of the psychological responses of victims to the prospects of legal intervention (pp. 98–106), which are placed in Kafkaesque relief with the views of those who have presumed, as well as argued against, the efficacy of legal remediation (pp. 106–8). Its advocates have become particularly disenchanted with its failures (pp. 109–16). In sum, the study contends that the legal stratagem for ending discrimination has not run well in double harness with history and needs drastic revision.

This is a well-organized study and one that is faithful to its governing premises. If it can be said that the study of power is the central concern of political science, Bumiller's effort is consistent with this ethic; but it primarily reflects a Foucauldian archaeology of knowledge. Dahrendorf's argument that problematic liberty is basic to the assertoric concept of liberty supports the study's contention that the legal paradigm has failed to establish the conditions essential to either. Unfortunately, Bumiller's dependence on the perspectives of these two theorists will not be apparent to those who are unfamiliar with the epistemology of Foucault or the sociology of Dahrendorf. The result is a veritable labyrinth of method and terminology, instead of clarity of thought.

Bumiller seeks to assay the social value of legal decisions, without supplying "new 'expert' advice about liberation strategies" (p. 113). The study's criticism is directed at convincing the reader that legal ideology is a device "for controlling and facilitating a new economy of power," which it does by projecting a "view of the victim's situation unencumbered by standards of objectivity, false responsibility, and legal criteria" (p. 113).

There are, of course, numerous and sufficient grounds for criticizing legal decisions on the subject of equality of treatment. Bumiller's tentative solutions leave one wondering about the scope of her vision, for implied in the skein of thought that runs throughout the study is a hospitable and uncritical acceptance of Dahrendorf's misconstruction of the relationship of natural law to theories of individual rights. A more cautious review of the historic role of natural law might have obviated what appears to be a condemnation of the political implications of the philosophy of natural rights. Instead, the study approves of the need for a theory of "group-based identities" to replace our present legal ideology's distortion of the "meaning of individuality" (p. 115). This kind of abstruseness has been encountered before and has with good reason been identified as an example of the equation of the intuitionist ethic of individualism found in theories of natural rights with the sanctifications of authority common to those of natural law.

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Making History: Agency, Structure, and Change in Social Theory. By Alex Callinicos. Ithaca, NY: Cornell University Press, 1988. 276p. $39.95.

This book is an example of "the remarkable penetration of English-speaking intellectual culture by Marxism over the past twenty years" (p. 2). It consists of the analysis by a Marxist, writing in the tradition of British analytical philosophy, of the concepts and propositions in the thought of other Marxists. The book could also be described as a philosophy of social science from a Marxist perspective.

As a Marxist and humanist, Callinicos tries to show that both social structures and individual human agents are ineliminable from the explanation of social behavior. This argument constitutes the basic theme of the book. Callinicos joins issue with methodological individualism, which wants to explain social phenomena solely in terms of individuals, and with the opinion that social-scientific explanation can and should be solely in terms of social structures, of which individuals are mere bearers.

On one level of analysis Callinicos is surely right. Individuals are not merely the products of the social structures in which they find themselves, but social structures do affect social behavior. As Callinicos argues, they empower or enable individuals within them to make history. If, however, we adopt the precise definition of methodological individualism that Callinicos quotes (from Jon Elster), we run into a problem with Callinicos' position.

According to Elster, methodological individualism consists of "the doctrine that all social phenomena . . . are explicable only in terms of individuals—their properties, goals, and beliefs" (p. 77). As an opinion about what is in principle possible, this idea is hard to refute; for the alternative is to maintain that social structures consist of more than the sum of their parts and that they have traits, necessary in explaining social behavior, that are not definable in terms of the individuals that make them up. Clarity of thought militates against this notion.

That it does so is a tenet of logical empiricism, with which many readers of this review will probably be sympathetic and which, as a philosophy of empirical science, seems to me to have much to commend it, although I share neither its epistemology nor its motives. Consequently, I wish to point out that three additional aspects of Callinicos' Marxism are inconsistent with logical empiricism.

First, Callinicos does not treat all scientific explanation as deductive-nomological. Rather, he holds with Max Weber that there is such a thing as "intentional" explanation, or explanation in terms of actors' reasons for their behavior. Second, and following from the first point, he does not insist that all concepts in social science be defined in terms of empirically observable phenomena (reasons are not empirically observable). Third, he denies the proposition that concepts are neither true nor false but are to be judged solely on the basis of whether their referents are empirically observable and whether they fit into valid explanations.

To the non-Marxist reader, the most interesting parts of the book will probably be its discussions of ideology and its relation to the superstructure of society. Callinicos does not insist that all ideology be false or that it serve the interests of the ruling class (being a Marxist he does not doubt that every society has a ruling class). Nor does he insist that every stable society have a dominant ideology permeating all its levels (and on this point he musters historical evidence).

What he retains from Marxist thought on ideology is the belief that the ideologies to which persons adhere are determined by their places in the economic structure of their society. Thus, he considers ideology as part of the "superstructure" of society, defined as all the noneconomic phenomena whose character is explained by the nature of the society's economic structure (p. 174). A society may thus have more than one ideology, and an ideology may play either a stabilizing, or a revolutionary, role.

The most surprising part of the book is the author's unflagging faith in the possibility and desirability of socialist revolution, despite arguments that the working classes of Western states are fragmented and declining in size and despite the appalling effects of such revolution, especially in the Soviet Union. His chief contribution in this regard is to argue against rational choice theorists who maintain that participation in revolution would be irrational for the individual proletarian.

Classicists might be taken aback by Callinicos' suggestion that Marx was an Aristotelian who valued virtue as contributing to *eudaimonia,* which Callinicos defines as well-being; and they might wish to see him deal with Eric Voegelin's assertion that Marxism is a fallacious attempt to "immanentize the Christian eschaton."

The book contains awkward attempts at inclusive language. The names of Michael Oakeshott and J. G. A. Pocock are misspelled. There is one four-letter word that one does not expect to find in scholarly discourse and too many typographical errors.

GLENN N. SCHRAM

The Moral Dimension: Toward a New Economics. By Amitai Etzioni. New York: Free Press, 1988. 352p. $24.95.

Neoclassical economics has become the utility infielder of the social sciences, good, presumably, for all seasons. Some reasons for its eminence and ubiquity lie on the surface: it readily generates refutable propositions; it works with whatever values and facts it is fed; maximization is easy to interpret; "self-interest" can be interpreted to rationalize whatever people do; methodological individualism goes well with the attribution of causal force to intentional actors; and market economics, to which neoclassicists lay claim, works, that is, increases economic growth far better than alternative modes of economic organization. Here, however, for the purpose of assessing the contribution of Etzioni's important book, I call attention to an attribute of neoclassical economists that is part of their enduring charm (and, when they persuade others, popularity)—the identification of economics with reason itself. Something that properly belongs to everyone, namely, rationality, has been appropriated exclusively for their own use by a tribe of true believers called economists and their followers in law, psychology, sociology, and political science. The purpose of Etzioni's seminal *The Moral Dimension* is to return rationality to its rightful owners—adherents of different ways of life.

Part 1, "Beyond Pleasure," is devoted to the most sustained and successful attack in the literature against the postulate of the narrowly self-interested individual without communal ties or affection for, and even with hostility toward, others. This is a faithful rendition of typical practice. Instead of allowing individual pleasure (what "feels good") to serve as the only source of utility, Etzioni would add moral commitment. Given that individuals evidence a variety of preferences, some based on shared commitments, the struggle to reconcile these competing commitments signifies "that these wants cannot be neatly ordered or regulated by prices."

This powerful argument might be strengthened further by observing that the competitive individualism modeled (or self-caricatured) in neoclassical economics is as genuine a way of life (or culture) as is hierarchical collectivism or communal egalitarianism or apathetic fatalism. You cannot bid or bargain or compete alone, as is shown by the ethnographic studies of the American Commanches and the Nigerian Ibos and Evans-Pritchard's justifiably renowned *The Nuer* (1940). Though it is eminently desirable to consider two ways of life, rather than only one (with its associated objectives and rationality), it is time, I believe, to go beyond individualism and hierarchy by adding at least egalitarianism and fatalism to our cultural kit bag.

The best and most original analysis, as readers of Etzioni's prior work might suspect, is contained in part 2, "Beyond Rationalism: The Role of Values and Emotions." Rational, that is, goal-directed, behavior, he argues convincingly, is impossible outside of supportive social relationships. Even those who stress cognitive limits on rationality overemphasize the lone individual's intellectual capacities. It is social life that gives the individual the opportunity for deliberation and correction so as to be effectively rational part of that time. Large and diverse bodies of evidence, which Etzioni uses well, reveal that most choices are made on the basis of value-guided emotion. As he says, "*non-rational choice is natural; rational decision-making is artificial;* it has to be manufactured" (p. 151, original emphasis).

My one disagreement with this brilliant analysis is the author's rejection of trial and error as a rational strategy, perhaps because I am less enamored of collective solutions than he is. Trial and error as deliberate strategy appears to him a form of adaptation, not rationality in the sense of knowing what one is trying to achieve. Would it not be instrumentally rational to prefer trial and error (knowing that in the past it led to better results) although one could not predict the paths to be taken than to prefer planning, which, while precisely specifying the path, was likely from experience to fail?

Whatever one thinks of this perennial debate, I would not give the neoclassicists (or, for that matter, anyone else) ownership of "rationality." Given their differing objectives, egalitarians, hierarchists, fatalists, and hermits are also rational in two important senses: they intend to secure their objectives and, by so doing, seek to support their preferred way of life.

"The more people accept the neoclassical paradigm as a guide for their behavior," Etzioni concludes, "the more the ability to sustain a market economy is undermined" (p. 250). Agreed. No institutions, no normative behavior, no community, no individual, no market. Thus Etzioni wishes to supplement neoclassical assumptions with what he terms "socioeconomics," in which values and commitments to others significantly shape behavior. Fine. But how many values and social commitments can there be? Are values disembodied, free to float anywhere? Or do they require people with social ties to support and sustain them? If they are infinite or large in number, the chances for a social science do not look good. If they are fewer, which ones are capable of attracting and sustaining social commitment? Or can there be commitments without values to justify them? With *The Moral Dimension,* Amitai Etzioni has taken us a step further toward a science that is also social.

Aaron Wildavsky

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Plough, Sword, and Book: The Structure of Human History. By Ernest Gellner. Chicago: University of Chicago Press, 1989. 288p. $24.95.

Grand histories reaching across all times and places remain distinctly unfashionable in the fragmented, cautious, and skeptical modern intellectual world. Maybe they deserve to be unfashionable. But Ernest Gellner, the distinguished British anthropologist and philosopher, has paid no attention. He has produced just such a history through an interesting mixture of broad anthropological generalization, schematic topical and historical organization, and pointed philosophical theory. Gellner intends his book to be a history of human life and society throughout their existence, a daunting project at which he may claim to have had considerable success. Gellner's undertaking, his history of the world, is daring, interesting, and valuable—if not always plausible—in its array of claims and insights.

Gellner is essentially interested in evolution in human history in what he apparently considers the major dimensions of social existence. He organizes our history into three familiar stages: hunting-gathering; agriculture; and modern, industrial civilization. In each Gellner concentrates on the central role and the history of three aspects: production, intellectual life, and coercion. The human story as it unfolds through him is less schematic, though, than this outline may suggest. It is, rather, dominated by complex interactive elements with nothing determined—but little a matter of mystery, either.

Gellner brings the skeptic of such history along with him for two reasons. Gellner writes in a surprisingly down-to-earth fashion that is usually effective. He evidently has written his book in the hope it will circulate beyond specialists. He avoids jargon and the other artifices of modern scholarship designed to make knowledge a tepid affair reserved for a few. His prose is open and inviting. At the same time Gellner's history teems with interesting observations, challenging perceptions, and large hypotheses (modestly presented). This is the real contribution he makes, not any particular theme or idea. In reading Gellner one reads not to buy some historical system (à la Toynbee or Marx) but to reflect through Gellner on human history, past and present.

Much of Gellner's discussion is very focused on the political, beginning, but hardly ending, with his considerations on the sword. Gellner is very much the "realist" in history, impressed with the presence and power of coercion in history even as he acknowledges its various forms and intensities. One of the many advantages of his provocative discussions of government, politics, and the military is his integration of them in larger social and historical contexts. Very much the comparativist student of politics, Gellner's work suggests the value of the long view and implicitly critiques the presentist narrowness and faddishness of much in contemporary political analysis.

Gellner's rich treatment ends with not-always-comforting reflections on our possible futures. They embody his consistent willingness to take a speculative (but not uninformed) road. They also reflect his low-key approach to so major an enterprise. At all points Gellner argues and interprets in an open-minded mood. He knows what he sees, but he is never dogmatic about it, which made both his enterprise and his perceptions far more fruitful for me than I expected.

At times one may complain that we get too little concrete history—or concrete historical examples—from Gellner. Philosophical interpretation does not negate the value of evidence or illustration. Moreover, Gellner's big picture has its inevitable tilts: history and Western history are very close to the same thing in his hands; family, sexual, and "moral" history do not rank in significance with production or coercion. But Gellner is the last person to fault for lack of ambition.

This is unconventional, possibly brave, history on a sweeping scale. That it is hardly a characteristic product of our minimalist age is yet one more reason why I recommend Gellner. He offers an energizing intellectual journey.

ROBERT BOOTH FOWLER

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Popular Culture and Political Power. By Fred Inglis. New York: St. Martin's, 1988. 270p. $35.00.

The New Politics of Old Values. By John Kenneth White. Hanover, NH: University Press of New England, 1988. 200p. $25.00 cloth, $10.95 paper.

What these two books have in common is an interest in popular culture and its relation to politics and society. Inglis has made the connection quite clear. His title indicates that this is the central focus of his book. White, on the other hand, has written a book on recent presidential elections and the Reagan presidency in particular. He discusses popular culture in various places and devotes a chapter, "Echoes in the Marketplace," to how popular culture "mirrors and amplifies" dominant values.

The books are different in a number of ways: Inglis is interested in culture theory and politics, White in presidential politics; Inglis is opinionated and theoretical, while White is impartial but certainly not "value-free."

The New Politics of Old Values is based on a great deal of research (60–120 footnotes per chapter); while *Popular Culture and Political Power* is, to a large degree, a discussion of the ideas of social and cultural theorists such as Clifford Geertz, Michel Foucault, Pierre Bourdieu, and Anthony Giddens.

Inglis doesn't think very much of politics and politicians. As he puts it, "Politics in the formal sense is mostly so awful and so boring; dismal, self-important people practise it, and give themselves such airs over us, who don't." (I wonder if he thinks the same about those who study politics?) White, on the other hand, is captivated by politicians, especially at the presidential level, and fascinated by politics.

Despite the differences, there is a common bond uniting these two works—both are concerned with how people go about making sense of the world. For Inglis what is crucial is how we find meaning in life, since this process, he suggests, shapes our political behavior. He stresses the need to "try for a politics of meaning which would provide a model of the production of meaning structures as they bind society to itself, and a model of decodings on the part of the audience, which enable them to understand and use these meanings as its subjects, citizens, participants, or victims." Ultimately he argues that "politics is coterminous with culture. Their study is mutually embedded."

White is also concerned with meaning and suggests that it is our "old values," such as individualism and egalitarianism that help people both find direction in life and decide who to elect as president. Given his interest in values, I found it curious that he did not cite important work on this subject such as Riesman's *The Lonely Crowd* (1950), Lipset's *First New Nation* (1963), and Bellah's *Habits of the Heart* (1985) or say anything about the work on values and life styles done at the Survey Research Institute.

Reagan's remarkable ability to identify himself with our dominant values, White argues, was one of the keys of his success. He also believes that how Reagan sold himself, by means of appealing to and reaffirming our fundamental values in his numerous stories, influenced our entertainment industry and popular culture. (Both authors believe that storytelling is an important means of transmitting values and culture.)

In this respect, he discusses the rise of Bruce Springsteen, the "myth" of Lee Iacocca, and values found in *The Cosby Show* in his chapter on popular culture. He suggests they all show a reverence for authority, generated, presumably, by the influence of Reagan on our value system. Curiously, White cites *Doonesbury* as having been affected by this current even though Trudeau has savagely ridiculed Reagan for years.

This raises an important point. Does popular culture always "mirror" our dominant values or does it function in varying ways for specific subgroups and subcultures? Does it always reinforce the dominant values or does it, at times, provide models for resistance? Does everyone get (decode) the same message from *The Cosby Show* or "Born in the USA"? There's good reason to suggest that they don't, which makes the matter of how popular culture relates to society and the political order quite problematical. Inglis seems to be more aware than White of these complications.

It is a sign of progress, I would say, that political scientists are focusing more attention on popular culture and the mass media. Inglis has devoted his book to this matter, but his style is so personal that it is difficult to find a core to his argument. He describes himself as "a middle-aged intellectual of devout domesticity, much good fortune, a passion for the landscapes of northeast England and the Mediterranean, and an absolute commitment to the . . . enduring values of the British Labour Party." The book is like a rambling stroll through his beloved English countryside. He offers all kinds of wonderful insights and has many interesting ideas. Those who like wide-ranging and discursive books will probably be pleased.

White's book is like a fast walk (there are only 144 pages of text in *The New Politics of Old Values)* between two points. He has digested a mountain of information but has not let it overwhelm him and has produced a highly readable and enlightening book.

Both authors have used popular culture for their own purposes, but that's a very common vice. Let any of us who is without "sin" in this matter start casting—or should I say *rolling?*—the first stone.

ARTHUR ASA BERGER

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Sovereignty and Liberty: Constitutional Discourse in American Culture. By Michael Kammen. Madison: University of Wisconsin Press, 1988. 256p. $25.00.

Michael Kammen is one of the most prolific and distinguished scholars writing today about U.S. intellectual and political history. His most recent book offers a collection of seven essays, all delivered previously as public lectures, exploring topics related to the core concepts of sovereignty and liberty in U.S. political discourse during the last two centuries. The essays are linked not just by these two specific themes but also by their continuation of a general interpretive approach staked out in Kammen's many earlier books, featuring attention to prevailing ideas and keywords that constitute the "exceptional" U.S. character, or "style," paradoxes and contradictions ("biformities") within this cultural inheritance, continuity and change in this dynamic tradition (as phenomena of great contemporary relevance), and the role of constitutionalism in U.S. political culture.

The chapters of the book range widely in their particular substantive focus. Kammen's first chapter reflects on changing perceptions of popular sovereignty during the years between the Declaration of Independence and the promulgation of the federal Constitution. He explores and modifies—yet ultimately confirms—the common view that leaders in the latter era were markedly more circumspect about popular self-government and "people power" than were the revolutionary activists. Chapter 2 analyzes much the same historical terrain from another angle by focusing on changing ideas of liberty and, to a lesser extent, equality in the young republic. These themes in turn provide the starting point for the third chapter's broad, historical sketch regarding the long evolution of prevailing ideas about "personal liberty," culminating in the modern judicial construction of a constitutional right of privacy, now much in jeopardy. The remainder of the book addresses debates more directly concerning formal institutional arrangements and practices of government. Chapter 4 offers a very interesting discussion that both demonstrates and justifies the important role of public opinion in evolving constitutional constructions by the Supreme Court. This discussion sets up parallel arguments exploring and defending the idea of a "living constitution" and a dynamic theory of "constitutional pluralism" in the fifth and seventh chapters. Sandwiched (somewhat awkwardly) between these overlapping discussions is chapter 6, reviewing the complex, erratic debate over states' rights during the two decades of rapid government nationalization prior to 1938.

All of these essays deal with questions and advance arguments that are very familiar among students of U.S. political thought, but each offers interesting new insights and empirical evidence regarding important matters of public debate. For example, Kammen's early chapters cover much familiar ground regarding the differences in revolutionary and constitutional era attitudes. His analysis is full of subtly original distinctions, however, including a fascinating analysis of the transition from prominent metaphors conceptualizing sovereignty as a "fountain of power" to the image of "power as a pyramid." Likewise, Kammen's later chapters manage to make interesting the otherwise tired debate about the relevance of the founders' original intentions to contemporary constitutional lawmaking by combining an acute wit, genuine respect for the logic of competing positions, and novel formulations of commonsense propositions. These insights, articulated in crisp prose and erudite style, make the book a notable addition to the literature on such debates.

The book is deficient in some regards, however. Some of the weaknesses are common to essay and lecture collections of this sort. On the one hand, the essays take up quite diverse issues only very loosely united around the book's title, thus giving the volume a rather diffuse character. On the other hand, the brief essays rarely probe very deeply into the murky pool of questions they identify: arguments tend to be underdeveloped, evidence is mostly anecdotal, and critical perspectives rarely reach beyond the terms of the mainstream discourse under scrutiny. In short, while the best essays here are interesting and mildly provocative, they lack the rigor, clarity, and depth of the author's best work.

Moreover, the author's general analytical framework and assumptions are sure to be less than satisfactory for many readers, including many scholars otherwise sympathetic to his specific substantive points and positions. For one thing, Kammen's straightforward interpretive approach, while refreshingly accessible and sensible in many ways, sidesteps many methodological and substantive theoretical questions of great concern to most critical social analysts. For example, the author's position here that ideas and the terms of discourse significantly shape history is—as in earlier works—stated as a matter of faith rather than explained and defended in compelling terms. Such silence—especially amid the vast and vibrant philosophical inquiry during recent years regarding the role of discourse and language as a form of power—renders his analyses curiously vague and elusive and less relevant to contemporary debate than they deserve to be. This silence is linked to a specific problem that recurs often in the author's discussions. Kammen laudably highlights at many points the complexity, multiplicity, and subtle changes in prevailing discourses over time as well as the fact that U.S. public opinion has rarely been uniform. However, he routinely slips into the argument that the opinions and debates identified among (mostly white, male, propertied, prominent) elites mirror the attitudes of society at large, a position that much recent study suggests grossly oversimplifies the broad range in experiences among differently situated citizens, especially among peoples marginalized or excluded by the dominant discourse.

In sum, this collection represents neither Kammen's best work nor any profound new directions in his general approach, but it does offer some gems of insight that should be of considerable value to scholars interested in the evolution of mainstream political discourse in the United States.

MICHAEL W. MCCANN

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Political Economy and the Rise of Capitalism: A Reinterpretation. By David McNally. Berkeley: University of California Press, 1988. 329p. $40.00.

Economic theory is frequently a form of political theory, justifying and setting limits to social relations and political possibility, often demanding one or another form of government. *Political Economy and the Rise of Capitalism* is a modest, though valuable, contribution to the growing analysis of political assumptions and prescriptions contained in economic theories.

McNally contends that most analyses of the origins and political content of classical political economy and capitalism are wrong in focusing on industrial and commercial sources. Comparatively examining British and French experience and theory, he plausibly argues that capitalism developed out of the agrarian revolution of the sixteenth, seventeenth, and eighteenth centuries that transformed rural social relations along capitalist lines. Placing the origins of classical political economy 150 years earlier than most theorists, McNally contends that this transformation provided the impetus for political-economic analysis from Thomas Mun and William Petty to Francois Quesnay and (more controversially) Adam Smith. Seventeenth- and eighteenth-century theorists, including Locke, attempted to explain, justify, and promote this development. Thus, "classical" political economists up to, and including, Smith focused on agriculture, were critical of industrial and commercial interests, and justified political and economic dominance by a rural, land owning, capitalist elite, repudiating "traditional" views that classical political economy developed from, and justified, the interests of industrialists and merchants.

McNally examines and illustrates the political purposes of early political-economic theories and elucidates the impact of social-political conditions on their development. French and British theory differed because Britain had already begun the capitalist transformation that French political economists argued must occur in France. Thus, in a process paralleling political theory, French economists proposed fundamental changes that British theorists took for granted. For example, the desire to strengthen the state led French theorists to argue for legal absolutism to introduce capitalist productive relations into French agriculture.

McNally's analysis of political assumptions and prescriptions and their interrelation with economics is much stronger on French theorists (especially the Physiocrats) than on British. Indeed, it is weak on seventeenth-century theorists such as Petty and even Locke and could be strengthened on Smith, to whom he devotes more than one-third of this book. Though he intelligently examines the relation between Smith's moral and political-economic theory (the normative basis of Smith's economics) and links him to the Scottish Enlightenment and civic humanism, he unfortunately slights Smith's explicit political assumptions and concrete role for government. In saying that Smith accords a vital role to government in economic development, McNally should further specify that role.

The traditional account of the origin of capitalism is set up as a straw person. Emphasizing agriculture is valuable, but his thesis about agrarian capitalism is not revolutionary and is stretched to fit Smith. McNally calls Smith an agrarian capitalist who argued for and justified rural, rather than industrial, development. Agrarian capitalism, however, is an elastic concept. Seemingly anything defending capitalist agriculture is agrarian capitalism, but they are not identical. Although some call Smith the precursor and apologist of modern, laissez-faire, industrial capitalism, many normative political economists deny this. That he discussed agriculture and is not an apologist for modern capitalism does not make Smith an agrarian capitalist nor divorce him from the industrial system that followed.

Despite McNally's admirable linkage between political-social-economic conditions and ideas, a richer appreciation of how available ideas limit what theorists see and say could strengthen his argument. Although Smith employs the agriculture images of his predecessors (political economists could not discuss industrial society before industrialization), subsequent theorists persistently found something more in his work. Although he did not defend an industrial middle class, Smith provided language and fictions useful for their interests. Numerous quotes criticizing manufacturers and merchants indicate both Smith's mistrust of their motives (which McNally fully discusses) and something more than the ideology of agrarian capitalism. McNally interprets Smith's criticism of behavior that violates natural liberty as criticism of manufacturing, but that does not follow. One may criticize manufacturers for not playing by ideal rules—as Milton Friedman does today—without attacking manufacturing. Smith defended an ideal many feared, full development of "natural liberty." He assailed mercantilism, not manufacturing; the desire for special protection, not trade; state promotion of specific interests, not implementation of general rules and institutional structures within which competition could occur. Even though Smith believed agricultural investment to be the most productive use of capital, he did not reject manufacturing. In prescribing a system of education to overcome deleterious effects of specialization on the masses, Smith acknowledged that manufacturing would expand. His general argument also provided part of the ideology of future liberal political-economists.

Political Economy and the Rise of Capitalism is useful, especially for the Physiocrats. McNally's interpretation of early political economy is a corrective to utopian beliefs in autonomous economic relations and to seeing only an "invisible hand" in contemporary polity and economy.

CONRAD P. WALIGORSKI

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Marxism and the Moral Point of View: Morality, Ideology, and Historical Materialism. By Kai Nielsen. Boulder: Westview, 1989. 288p. $38.00.

Judging from the book at hand, Marxism as a plausible worldview has fallen upon hard times. In the East, as Milovan Djilas notes, Marxism is both dead and very much alive: dead at the level of theory but very much alive at the level of justification for everyday totalitarian practice.

Kai Nielsen's work gives voice to an internal dialogue among Marxist analytic philosophers that reflects the crisis of Marxist theory in the West. Western Marxists are increasingly arriving at conclusions like those of their Eastern counterparts who take Marxist theory seriously, namely, that Marx's theory or canon is seriously flawed and hence that it is in need of a major overhaul.

Nielsen's book represents such an attempt at a major overhaul of the Marxist intellectual heritage focusing on the crucial issue of ethics-morality and justice. Where Nielsen succeeds admirably is in his conceptual distinction between Marxist moralism and immoralism and his innovation of a tertium quid, a position that claims both scientific and objective grounding for a Marxist *moral* point of view in contradistinction to mere moralizing and moral absolutism.

To accomplish such an ambitious undertaking, Nielsen is compelled to reinterpret some of the major sinews of Marx's conceptual edifice: historical materialism, economic determinism, base and superstructure, praxis, the class nature of morality, ideology, conflict, class struggle, egalitarianism, socialism, and the classless society (communism). In reconsidering the writings of Marx, Engels, Lenin, and some contemporary Marxist theorists and Marxologists, Nielsen doggedly adheres to Marx's imputed fundamental values, while critical of his fellow Marxist colleagues' dissenting voices.

Throughout his overly repetitive work (which includes previously published essays) Nielsen postulates the ambivalent thesis that *if* Marx's account of this or that phenomenon is reasonably accurate, we can conclude with Marx that, for example, capitalist society is unjust, socialism is a more just social system than capitalism, and communism as a classless (though not entirely conflictless) society represents the best of all possible worlds. Thus, Nielsen asks rhetorically: "Can Marxists reasonably claim, particularly if their descriptive-explanatory-interpretive claims are approximately true, that capitalism is unjust and that socialism is just or at least less unjust?" (p. 276).

What one misses most in Nielsen's volume is, indeed, a comprehensive, or even an indicative, account scrutinizing the ambivalences and the pitfalls of Marxism both as theory and practice. It would appear intellectually, if not ethically, inadmissible to theorize about Marxism in the late twentieth century without at all considering the accumulated scholarship, both Marxist and non-Marxist, critical of Marxism, let alone the overwhelming practical evidence of everyday reality of decades of socialist (read *Marxist-Leninist)* regimes that have caused untold human misery, death, and suffering, and whose theoretical principles have led to the material and spiritual impoverishment of people wherever men have attempted to put them into practice.

Absent from the Nielsen volume are even references to such critics of Marxism, socialism, and communism as Hannah Arendt, Daniel Bell, Peter Berger, Mark Blaug, Eugen von Böhm-Bawerk, Djilas, Carl J. Friedrich, Zbigniew Brzezinski, Nathan Glazer, André Glucksmann, Václav Havel, Eugene Kamenka (*The Ethical Foundations of Marxism* [1972]), George L. Kline, Pavel Kovaly, Bernard-Henri Lévy, Ludwig von Mises, Robert Nisbet, Michael Novak, Ortega y Gasset, James J. O'Rourke, Paul C. Roberts, Matthew A. Stephenson, Adam Schaff, Alexsandr I. Solzhenitsyn, Alexis de Tocqueville (on liberty versus equality), Jacob L. Talmon, Bertram D. Wolfe, and the original Marxist-humanist Yugoslav theorists of the famed "Praxis" group such as the ethicist, Svetozar Stojanović. Other critics such as Milton Friedman, Friedrich Hayek, Sidney Hook, Arthur Koestler, Leszek Kolakowski, and Karl Popper are mentioned only in passing. Hayek's *Law, Legislation, and Liberty* (1973-79) and Kolakowski's *Main Currents of Marxism* (1978) are not even mentioned.

It is a laudable trait of Nielsen as a scholar almost always to give the benefit of the doubt to those like Marx, Engels, and even Lenin, with whom he agrees. It may have been also beyond the scope of a single volume to explore more fully—that is, in an interdisciplinary way—the many ramifications of the notions of ethics-morality and justice in the Marxian vision of the perfect society and its critique of the capitalist mode of production. But to ignore, for example, the Neo-Austrian critique of Marxist economic theory and the theory's repudiation by everyday reality in "real existing socialism" is to be less than fair, realistic, practical, and scientific. One might even argue that Nielsen's methodology is un-Marxist, since it ignores Marx's injunction concerning the unity of theory and practice. It also flies in the face of the author's own stated "contextualist" paradigm (pp. 8-12).

Nielsen's philosophical reductionism may or may not reflect the poverty of Marxist analytic philosophy. There is little to recommend it to either theorists or practitioners of the historical *novum*. Yet the volume's redeeming feature is its very focus on the question of ethics and values that necessarily underlies the architectonic systems of such major theorists as Karl Marx.

OSKAR GRUENWALD

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Reason, Ideology, and Politics. By Shawn W. Rosenberg. Princeton: Princeton University Press, 1988. 250p. $37.50.

Political Reasoning and Cognition: A Piagetian View. By Shawn W. Rosenberg, Dana Ward, and Stephen Chilton. Durham, NC: Duke University Press, 1988. 193p. $32.50.

For many years, ideology was an alien concept in U.S. political science. When some behavioral political scientists made ideology a focus of their empirical research in the 1960s, they did so within the framework of two assumptions: (1) that ideology could be defined as a network of beliefs conceptually distinct from science or, more generally, from a rational portrayal of reality; and (2) that ideologies could be studied through techniques designed to elicit statements from individuals expressing their opinions regarding a variety of (mostly political) issues. Neither assumption has stood the test of time or criticism very well. The first proposition was an early casualty in the antipositivist attack on the epistemological claims on which the validity of the conceptual distinction between ideology and science was premised. The second assumption appears to have drowned in a sea of banalities, for behavioral political science has not managed to say very much of interest concerning ideology during the last 30 years.

Shawn Rosenberg and colleagues, authors of the two books under review, take this situation as a starting point for their own study of ideology, and, not surprisingly, they call for the abandonment of the fundamental assumptions underlying belief systems research. The empirical study of ideology, they argue, "tells us very little about how most people think about politics" *(Political Reasoning,* p. 28; see *Reason, Ideology,* p. 1). Operating with the individual's belief as the unit of analysis, this research has not generated a theoretical notion of ideology; nor has it accounted for the instability and variability of individual attitudes or their unreliability as predictors of individuals' actions *(Reason, Ideology,* pp. 3-6, 44-48; *Political Reasoning,* pp. 26-30). "The current state of affairs," according to Rosenberg, "suggests the need for . . . a new perspective" (p. 2). The problems inherent in the existing research, both books insist, lie on a deeper level than that reflected in most criticisms of behavioralist methodology. The true source of the difficulty is that contemporary political scientists "lack any formal or abstract concept of ideology" to guide their research *(Political Reasoning,* p. 32). It follows that "a new theory of ideology must be developed" *(Reason, Ideology,* pp. 59, 200).

This new concept of ideology rests on the assumption that different individuals think differently about politics and that ideological thought is a coherent structural unity. In order to appreciate the force of the first point, differences in perspective must not be seen as the consequence of objectively determined environmental conditions. Rather, the authors insist, "the subjective dimension of thought and action" must be emphasized *(Political Reasoning,* p. 2; see *Reason, Ideology,* pp. 12-14). Since meaning is subjectively created by the individual, research on ideology, they argue, must direct attention to the psychological rather than to the sociological dimensions of political thought *(Reason, Ideology,* pp. 78, 80, 82, 97). Political thinking ought to be viewed as "a subjectively directed and self-structured activity" (p. 59).

On the other hand, they maintain that ideology should not be defined as a partial and distorted representation of an objective reality. Instead, ideologies can be conceived as integrated systems of rationally held beliefs, provided that one accepts the general proposition that all thought displays certain formal qualities or "deep structure" *(Reason, Ideology,* pp. 10-11, 51; *Political Reasoning,* pp. 8, 11-12). Such a view undermines the distinction between science and ideology according to which the former claims a superior status to the latter on the basis of its formal methodological principles. Taking these two primary propositions together, the emphasis in these two books is on "the formal structure . . . of individuals' political thinking" *(Political Reasoning,* p. 14; see *Reason, Ideology,* p. 22).

Foundational support for this commitment is supplied by the work of Jean Piaget. Both books rely upon the concepts and arguments of Piaget's developmental psychology in order to explain the origins and structure of different types of thinking, although only *Political Reasoning and Cognition* offers the reader a summary account of Piaget's research. The authors maintain, however, that Piaget's psychological framework must be modified to take into account certain sociological factors. Hence, "our notion of social environments and their role in conceptual and ideological development goes well beyond Piaget's own thinking" *(Political Reasoning,* p. 13; see *Reason, Ideology,* p. 2). Perhaps, but the sociological aspect of the works under review consists largely of vacuous generalities such as, "Structurally different sociocultural contexts . . . foster different levels of individual development" *(Political Reasoning,* pp. 91, 93). What does receive detailed consideration in these books are the "psychological dimension" and the formal structure of political thinking (*Political Reasoning*, pp. 1, 14). Thus, *Reason, Ideology, and Politics* and *Political Reasoning and Cognition* build on three typologies of thinking—linear (causal), sequential (descriptive), and systematic (analytic)—which the authors employ to interpret the responses of their research subjects to political and nonpolitical questions in support of their conclusion that political logic (and meaning) varies from person to person (*Reason, Ideology*, pp. 49, 100–157, 165–80; *Political Reasoning*, pp. 100–126).

In critically assessing these two books, one hardly knows where to begin in terms of enumerating the arguments not thought through by the authors. What we need, Rosenberg, Ward, and Chilton write, is an epistemologically grounded set of analytical categories, that is, Kantian epistemology (minus the transcendental ego) deployed on behalf of a theory developed without reference to any specific empirical observation, whose concepts are self-validating (*Political Reasoning*, pp. 37, 39; see *Reason, Ideology*, pp. 51–52). Considering the importance of this epistemological claim to the general argument of both works under review and the ease with which the authors dismiss Lockean epistemology, it is disappointing, to say the least, that they evince no recognition of the philosophical difficulties posed by the defense of such a claim. Nor, in suggesting that an ideology will prove unworkable "insofar as the structure of an individual's thought is inconsistent with the structure of environment," do the authors explain how, or according to what evidence, the "objectively structured world" is to be determined (*Reason, Ideology*, pp. 12, 19; see *Political Reasoning*, pp. 8–9, 12).

The notion that an equilibrium or goal "is reached when the structure of the individual's thought parallels that of reality" (*Political Reasoning*, p. 9)—(one should never break the window in a locked room?)—is the kind of ridiculous statement that one expects to hear from gurus or mystics, not from social scientists who should know better. Similarly, the assertions that collectively defined norms and institutions can be better understood as an infinite number of subjectively directed and self-structured activities, so that "it makes no sense to talk about a single organizational framework" must come from someone who has never been in the army (*Reason, Ideology*, pp. 12, 55, 59; see *Political Reasoning*, pp. 17–18). That sequential thinking appears to express the cognitive needs of tribal society, linear thinking the needs of feudal society, and systematic thinking the needs of modern industrial society reproduces in the grossest manner the ethnocentrism and unsupported assertions about "primitive mentality" that characterized the early formulations of structuralist theory (*Reason, Ideology*, pp. 135, 153–54; *Political Reasoning*, pp. 146–53). In works that purport to take into account the sociological dimensions of ideology, it is a bit disconcerting to find the authors declaring that a capitalist market economy is characterized by "mutual respect and rational debate" and that the cognitive needs of individuals are fulfilled by the institutional structures of contemporary society (*Political Reasoning*, p. 152; see *Reason, Ideology*, p. 154).

What contemporary political scientists need are not formal abstract concepts of ideology or psychologistic assertions about subjectively constructed meanings but rather a methodological approach that is sensitive to the contextualist and situational features of political thought and action. Behavioralism's failure to capture these aspects of politics because of its search for empirical generalities and its ahistorical conception of the data provides no pretext for moving in the opposite direction, that is, towards timeless Kantian concepts and an atomistic deconstruction of sociological phenomena. Until its sociologically generic and historically specific characteristics are accorded sufficient emphasis, ideology will continue to exist as an elusive concept beyond the grasp of political science.

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Weber, Irrationality, and Social Order. By Alan Sica. Berkeley: University of California Press, 1988. 350p. $32.50.

Weber effectively distanced his thought from both Hegelian and Enlightenment forms of rationalism, as well as the romantic accentuation of irrational experience by conceiving rationality as a "world of different things."

Human reason can be, and has been, applied in different ways to thought and action in every realm, including religious, aesthetic, and intellectual spheres far removed from capitalist and bureaucratic calculations of abstractly defined means and ends. So expansive are Weber's polyvalent accounts of rationality that they seem to absorb irrationality itself, thereby obscuring its anchorage in human dispositions and temperament: "A thing is never irrational in itself, but only from a rational point of view."

Alan Sica is not the first to recognize inconsistencies and tensions in Weber's thought and biography signaling or suggesting that his lifelong struggle to confine irrationality within cognitive bounds did not always meet with success, but no other commentator has interpreted Weber's "irrationality problem" with such well-documented perceptions and imaginative explications. Sica advocates and practices an "aesthetic management of ideas," combining a sensitivity to the elusive and ambiguous, yet telling, detail; a relaxed, digressive style; and a deep erudition—all worthy of a first-rate litterateur. Occasionally, he loses his way toward a point he is trying to make, and his immersion in Weberiana sometimes crosses the border into unwarranted pedantry; but the strategy succeeds by enabling him to treat profound themes with a highly refined nimbleness and zest.

While much of the book involves close, contextually informed readings of selections from Weber's texts, it is in no way a conventional exegesis. Indeed, many seemingly relevant Weberian themes, such as the fragmentation and disenchantment of public life, surface relatively infrequently; while influential conceptual reconstructions of Weber's accounts of rationality (e.g. Kalberg, Levine, Schluchter) are swept beneath the carpet without mention (p. 194). Sica's hermeneutic ambition is to use his interpretation of Weber's struggles to suppress the irrational in his works to provoke recognition and reflection on the same tendencies in modern schools of thought. Instructive comparisons with forgotten thinkers (especially Pareto), critical obiter dicta, and programmatic recommendations for new methods and themes interrupt the interpretations and in several chapters overtake them altogether.

Compelling evidence of Weber's "irrationality problem" is adduced from his methodological and historical writings, as well as his ideal types. However, the most basic and incisive datum is that Weber's fundamental conception of social action lacks a metapsychology of the irrational dispositions that animate behavior. This observation necessarily presumes a more explicit comprehension of irrationality than Weber acknowledged or employed. Sica artfully illustrates, complicates, and qualifies his sense of irrationality throughout his work; but it can be distilled into three essential points: (1) irrationality consists in habits and impulsive motives expressed in forms of belief and behavior over which actors do not have full conscious control; (2) irrational behavior or belief controls actions in gaining pleasant, unpleasant, and ambivalent results, which infuse social life with existential and cultural meaning; (3) critical choices aside, irrationality overwhelms theoretically postulated pure rationality in both bulk and significance during the ordinary course of behavior.

There is enough solid insight in each of these points to suffice as hermeneutic suppositions, but the overall effect—only partially mitigated by qualifications in the text—is to accentuate the salience of irrationality as if to overcompensate for Weber's suppression of concern. Cognitive capacities to reason and associated powers of sublimation and rational restraint may be extraordinarily well-developed in figures such as Weber, but they are not the exclusive possession of "heroic" intellectuals as Sica implies (p. 162). If Sica overcompensates on this issue, he stays too close to Weber on the centrality of meaning in social life. By stressing the irrational grounds of meaning, Sica hopes to move beyond associations between irrationality and evil that extol reason as the only good. Yet granting that irrationality has potential for virtue should not blind us to its frequent association with tragic fate. This connection is hardly confined to classical literary works. Sica could have made much of the fact (but does not) that nothing in Weber's conception of social action prepares us for world-historical tragedies, such as Jacobinism, Stalinism, and the Nazi "final solution," which in some measure stem from impulses over which key actors did not have full conscious control.

The upshot here is that the metapsychology Weber omitted must move beyond dichotomies of rationality and irrationality, control and fate in directions that are not as yet clear. Doing so will be no simple task. Sica notes that even Pareto found irrational "residues" too complex to explain (p. 174, n. 17). Freud and Marcuse oversimplified these issues, as have most of their successors.

Sica bears only a limited responsibility for a metapsychology he did not set out to write. He has accomplished a great deal by making irrationality theoretically germane. Studies of the masters of social thought rarely fuse textual and contemporary horizons so well.

IRA J. COHEN

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AMERICAN POLITICS

The Politics of Capital Investment: The Case of Philadelphia. By Carolyn Teich Adams. Albany: State University of New York Press, 1988. 202p. $39.50 cloth, $12.95 paper.

America Becomes Urban: The Development of U.S. Cities and Towns, 1780–1980. By Eric H. Monkkonen. Berkeley: University of California Press, 1988. 336p. $25.00.

City, State, and Market: The Political Economy of Urban Society. By Michael Peter Smith. New York: Basil Blackwell, 1988. 256p. $34.95.

In too many instances, the argument that "politics matters" signals premature closure rather than the introduction of a theoretically grounded analysis of the politics of place. To some extent, this lacuna reflects the mesmerizing effects of a rich set of stimulating but competing and fragmentary theories of the nature of global economic restructuring trends. There is a consensus that such trends dramatically alter local economic and political choices but interpretations of restructuring tend to be descriptive and essentially apolitical, with few clues on how global economic changes relate to local political life.

These three authors provide some intriguing leads in a remarkably congruent set of themes and concerns. Foremost is a common interest in approaches that more systematically integrate the role of human agency with the economic and technological forces molding urban development. At different levels of abstraction, they also underscore the normative implications of changing governmental intervention strategies at national and local levels and the significance of public and private sector organizational incentives in shaping local political choices. This attention to the roles of ideology and institutional change in state and market relations distinguishes these studies from more limited analyses of urban political change.

Overall, there is a consensus that insufficient theoretical attention is given to the political arrangements and processes through which localities engage and "translate" economic restructuring processes. Monkkonen sees this as a chronic oversight in urban history. His "sketch" of two hundred years of U.S. urban history critiques functionalist, social control, and "new urban history" paradigms for their failure to produce a "truly city-focused urban history" (p. 245); and he goes on to skewer the aesthetic biases of Lewis Mumford, Jane Jacobs, and other urbanist icons, a gratifying deed for those of us who secretly prefer Los Angeles to San Francisco. In their stead, Monkkonen advocates historical analysis that is more sensitive to formal organizations as expressions of local social relations and political choices.

He amply illustrates the importance of such organizations in the transformation of U.S. cities from regulatory to service-providing entities. But his historical account to local organizations and urban growth strategies is especially timely. He emphasizes the mutual interaction of technological development and political organization, pointing out the many ways in which local groups competed for, and subsidized, new technologies. This historical vision of economic and technological change is an important corrective for analyses that depict transportation, communication, and information technologies as determinative of past or present urban development processes. Similarly, Monkkonen's analysis of U.S. cities' historical entrepreneurial roles will be instructive for those dazzled by the seemingly innovative strategies of modern U.S. cities. Yet theoretical development of this relationship between formal organizations and urban social change is limited. Although Monkkonen is convinced that organizations matter, his argument is diminished by the lack of precision in conceptualizing organizational structures and processes and the failure to specify, in any systematic way, how these features interact with the mix of locational and personal factors that Monkkonen sees as shaping urban futures.

Smith's treatise moves beyond structuralist accounts to illustrate the exceptionalism of the U.S. welfare state and to incorporate ideological hegemony as a source of political dynamics. In a lucid and cogent presentation, he points out that scholars tend to focus on state expenditures rather than the political interests and policies creating a drain on state revenues. In redressing this imbalance, Smith traces the emergence of a fiscal welfare state in the United States, a tax state whose market capitalism ideology, off-budget spending, and "military Keynesianism" result in depoliticization and disenfranchisement at the local level. As national urban programs are reconstituted toward less-visible tax privileges for private investors, the fiscal base for financing public services erodes, as does the political standing of previous beneficiaries. Both Smith and Adams stress that this elusive change in the form of state intervention at the national level will transform the quality and content of local political life. But where Smith's theoretical discourse is replete with maddening references to "urban managers" and elites, Adams's approach details how diverse administrators and politicians respond to these changing incentives.

Adams's analysis of capital investment patterns in Philadelphia over a 30-year period deftly incorporates these organizational and ideological themes in an exemplary model of theoretically informed empirical research. In contrast to the assumptions underlying most urban political theories, Adams finds that Philadelphia's allocation of community facilities is evenhanded, sometimes generous, to poor neighborhoods and does not systematically favor downtown areas at the expense of neighborhoods. She attributes these counterfactual findings to planning ideologies, particularly the conviction that public investment can stimulate private redevelopment and social reconstruction of communities. Organizational incentives favoring targeted public investments and rehabilitation rather than new construction may also benefit poor neighborhoods.

Yet Adams sees public authority over capital spending and the potential for redistributive policies threatened by the emergence of two local political arenas, delineated by different revenue sources and characterized by distinctive political processes. One involves community facilities and social expenditures supported by general tax revenues and another deals with large-scale capital improvements funded by private capital markets, federal tax expenditures, and federal program subsidies. For many cities like Philadelphia, the "financial facts of life" increasingly gear capital spending towards the dominance of the second arena and the institutional priorities of these external revenue sources.

Smith and Adams concur that trends toward privatization, off-budget spending, and public-private partnerships effectively reconstitute urban policy as tax policy and reduce state capacities. Furthermore, these trends shift local revenue issues to a national political arena dominated by Congress, the Treasury, and the Office of Management and Budget. Although Smith anticipates local collective action in resistance to these developments, Adams contends that the restricted ability of local constituencies to influence national off-budget policy making implies both depoliticization and nationalization of local fiscal policy. Similar trends in Europe indicate that comparative analysis of these ideological and institutional changes will be a key to understanding one of the more compelling ironies facing urban scholars.

SUSAN E. CLARKE

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JFK: History of an Image. By Thomas Brown. Bloomington: Indiana University Press, 1988. 160p. $19.95.

John F. Kennedy: Person, Policy, Presidency. Edited by J. Richard Snyder. Wilmington, DE: Scholarly Resources, 1988. 178p. $35.00.

The circumstances of Kennedy's death, not the events of his presidency, help create the "myth" of Camelot, charisma, and "What if he had lived?" Kennedy's assassination was a traumatic experience for most citizens. An attractive, vital man had been struck down by an assassin's bullet. Surveys later showed that a significant portion of the adult population reported being unable to sleep or eat. Many felt as though they had lost a family member. Over 25 years later, opinion polls report that John F. Kennedy is among the most retrospectively popular presidents in history.

These two books address the prevalent images of the Kennedy years. In *JFK: History of an Image* Thomas Brown explores the several images of John Kennedy that have emerged over the last 25 years and what the images reveal about U.S. society and politics. The strongest analysis rests in Brown's bibliographic essays and not in its application to broader theoretical issues. Brown reviews the "hagiography" of early adulatory work that created the myth of JFK as an idealistic hero of U.S. reform. These first custodians of Kennedy's legacy included Evelyn Lincoln, Arthur Schlesinger, Jr., and Theodore Sorenson. Each was extended extraordinary cooperation by the Kennedy family and, in return, agreed to selective censorship. For these keepers of the flame, Kennedy emerged as a man of reason, style, elan, inner strength, and growth.

Brown then reviews the revisionist literature on Kennedy, which tended to focus on foreign affairs. Kennedy is seen as embodying the ideological limitations of U.S. liberalism by failing to question the underlying premises of U.S. politics. Vietnam was viewed by Kennedy as a test case for counterinsurgency; and nowhere did the public or private record, save for the recollections of custodians, provide evidence of a Kennedy withdrawal from Vietnam.

From Kennedy revised Brown moves to Kennedy revealed. Sexual misconduct, private life and character, gangsters, and a macho obsession with destroying Castro are part of the Kennedy revelations. Brown concludes with a brief discussion of current scholarly work on Kennedy that offers a more detached, historical perspective.

John F. Kennedy: Person, Policy, Presidency is an edited volume of essays derived from a conference at the University of Wisconsin, La Crosse, attended by former Kennedy aides and historians. The volume begins with an interview with Arthur Schlesinger, Jr. and is followed by essays from several historians including William Leuchtenburg, Bruce Mazlish, William Chafe, and Allen Matuson. There is little cohesiveness to the essays and not one of them includes a citation, leading the reader to wonder why such instructions would have been given to the contributors. The topics covered include the array of foreign and domestic crises that characterized Kennedy's administration. The volume does little to help us better understand either the myth or the record of the Kennedy years. Scholars of the presidency would be wise to start with Brown's literature review and not be sidetracked by Snyder's rather bland and uninspiring volume.

LARRY BERMAN

University of California, Davis

The Rise and Fall of the New Christian Right: Conservative Protestant Politics in America 1978–1988. By Steve Bruce. New York: Oxford University Press, 1988. 224p. $34.50.

The appearance of religious interest groups like the Moral Majority as political organizations is one of the most important of the recent changes influencing U.S. elections. In this book Steve Bruce explains the rise of the New Christian Right; evaluates its achievements; and shows why, despite some local successes, it failed to remake the United States into the vision of *one nation under God*. This study of the sociology of social movements by Bruce is the third in his series of detailed empirical examinations of Protestant politics in various settings. The first two dealt with anti-Catholicism in modern Scotland and with Paisleyism in Ulster.

Unlike other books on social movements that use status defense and irrationality as a framework for understanding conservative political behavior, Bruce presents social change in the New Christian Right case as one where "resources" (members, funds, publicity, etc.) were mobilized in response to some grievance. Leaders play a significant role in the mobilization process, so that much attention is given to the roles played by people like Jerry Falwell and the Moral Majority, Pat Robertson and other televangelists, and the "Holy Trinity" of professional right-wing organizers (Richard Viguerie, Howard Phillips, and Paul Weyrich) in early New Christian Right successes.

The material for the study comes from a variety of published and unpublished sources, especially interviews with New Right activists and opponents. Bruce explains the emergence of the movement as a reaction to the liberal and cosmopolitan penetrations of U.S. culture by secular society. He traces its demise to the inability of the New Right Christians to work with conservative Catholics, Jews, and Mormons and to schisms within conservative Protestantism itself.

As an Englishman, Bruce is more impressed with the openness of the U.S. federal system than most U.S. readers are likely to be. He finds much of the success for the New Christian Right in the diversity of a system that enabled Christian activists to take over local Republican organizations and air their opinions on neighboring media markets: "The open nature of the American legislative systems allows well-organized interest groups . . . to introduce laws which will protect or promote their particular culture, and to make their agenda an election issue" (p. 125). In the end, Christian activists had to settle for regional victories instead of a national restructuring.

The book is a brief analysis of a decade strewn with religious conflict, and as such it is admittedly incomplete. In the discussion of abortion the role of Protestant activists like Francis Schaeffer, C. Everett Koop, and various charismatic personalities such as David Wilkerson would strengthen the leadership thesis and improve the analysis. The reliance on the publications and behavior of the Moral Majority as the prototype of a New Christian Right group weakens the analysis by dismissing the role various denominations and other interest groups, like the National Association of Evangelicals, played in the drama of the eighties. Still, these shortcomings do not detract from a book that is both analytical and insightful.

Bruce finds that any analysis of the New Christian Right is admittedly overblown, since its influence has been greatly exaggerated by both enemies and friends. The strength of the book may be in Bruce's conclusion that the failure of the New Christian Right's grand designs for cultural reformation may lead to a new strategy of "claiming social space for its beliefs, values, and practices on the grounds that it represents a legitimate minority which the modern state discriminates against" (p. 182). Perhaps such a strategy will be in evidence in the next decade.

J. DAVID WOODARD

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The Politics of the U.S. Cabinet: Representation in the Executive Branch, 1789–1984. By Jeffrey E. Cohen. Pittsburgh: University of Pittsburgh Press, 1988. 208p. $34.95.

Jeffrey Cohen has made a substantial contribution to the presidency and public administration literature with *The Politics of the U.S. Cabinet.* However, this study may not be to everyone's taste. Cohen's approach is uncompromisingly quantitative as he seeks to redress the reputedly anecdotal and descriptive methodologies of previous cabinet studies. Cohen's work is also informed by an explicit theoretical perspective as he seeks to place the cabinet within a framework of interest group politics. Indeed, the case against the cabinet as an institution and the case for its important representational role in a democracy provide the foci of Cohen's argument.

Although his treatment is hardly a traditional historical one, Cohen does survey the cabinet over the two-hundred years of its existence. In doing this, Cohen has ingeniously amassed an impressive array of statistical data. In over 50 tables, with information culled from the *Biographical Directory of the United States Executive Branch, Who's Who,* and the *Directory of American Biography* (to cite a few of Cohen's archival sources), the reader finds extensive data on the 468 individuals who have served as cabinet members from George Washington's administration through 1984. By organizing these demographic data into five historical eras, Cohen provides interesting longitudinal, as well as summary, analyses of the age, education, region, occupation, partisan activity, and electoral experience of the secretaries. Additionally, statistical tests of significance are employed in a novel (if somewhat unconventional) manner to provide a basis for judgments about the observed differences.

Fully one-third of *The Politics of the U.S. Cabinet* (chapters 3 and 4) deals with detailed and thorough elaborations of cabinet members' characteristics. Indeed, providing exact information about the social backgrounds, political experience, and tenure in office of cabinet secretaries is one of Cohen's most important contributions. Cohen's book will doubtless become the standard reference for any future textbook discussions of the cabinet. On the other hand, one might question the utility of overelaborated analyses of fairly commonplace observations. For example, it is noted that the average age for a cabinet secretary on appointment is 51.8 years and that this has been fairly constant over party eras except for the founding period (1789–1824) when cabinet secretaries were younger (43.6 years). Furthermore, while the average tenure of a cabinet secretary is 38 months, those who served in the earliest party era averaged 69 months in office. Clearly, revolutionary eras are different from later eras. However, Cohen would have us reason as follows: "A multiple regression was run of the form Y (length of service in months) = constant + B_1 (age) + B_2 (party era). Here a dummy variable was created for party era such that 0 equals era 1 and 1 equals eras 2, 3, 4, and 5. I ran other equations with dummy variables for each of the party eras, but the equations showed that the only real difference among them was that between the first and all the others" (p. 55).

The point here is that despite the disclaimer about "statistical razzle-dazzle" (p. 91) being unnecessary to reach most conclusions, there is a great deal of methodological overkill employed to make some very basic points. The methods have a usefulness, but there is a danger with allowing statistical pyrotechnics to replace the personal realities of cabinet politics and personnel. Rarely does the mention of a particular department or official intrude on Cohen's discussion. Admittedly, the goal is to transcend the anecdotal with the analytical; but the outcome is to render one of the most interesting of U.S. political landscapes devoid of its practitioners and their policies. Surely, some discussion of Hamilton at Treasury, Hay at State, Hoover at Commerce, Ikes at Interior, and McNamara at Defense would have been helpful. Leonard D. White, whose name is absent from both index and bibliography, may still have something to tell us about the practice of public administration.

However, this may be to criticize Cohen for the book that he did not intend to write. Jeffrey Cohen has written a tightly constructed, carefully argued, methodologically sophisticated, and historically grounded study of an important U.S. political institution. That the cabinet is not an "institution" possessing definite boundaries and administrative autonomy (see chap. 2) presents Cohen with the opportunity to argue, if inconclusively, for its important representative functions. On the other hand, Cohen might have examined structural rearrangements—such as an executive cabinet or formalized cabinet councils—that have been proposed to redefine "cabinet government." Despite some shortcomings in approach, *The Politics of the U.S. Cabinet* is a significant work of original research that will be frequently cited in any future studies of U.S. executive organization.

KEVIN V. MULCAHY

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Clouds of Secrecy: The Army's Germ Warfare Tests over Populated Areas. By Leonard A. Cole. Totowa, NJ: Rowman & Littlefield, 1988. 188p. $21.50.

Leonard Cole's *Clouds of Secrecy: The Army's Germ Warfare Tests over Populated Areas* is in some ways mistitled. In addition to an account of the tests mentioned in the title, it briefly examines the recent history of biological weapons and, in particular, describes the U.S. biological weapons program. The book also explores some of the implications of new genetic technology for biological weapons and evaluates the charges that the Soviet

Union has violated the 1972 Biological and Toxin Weapons Convention. There are books covering the recent history of biological weapons in more detail, notably Charles Piller and molecular biologist Keith R. Yamamoto's *Gene Wars: Military Control over the New Genetic Technologies* (1988), which also adds material on biological weapons arms control. Nevertheless, *Clouds of Secrecy* is unique in the information it provides about the U.S. army's vulnerability testing program in the 1950s and early 1960s.

The book's core is a readable account (suitable for undergraduates or lay audiences) of examples from one of the more bizarre episodes in military history, the army's biological weapons vulnerability program in the 1950s and 1960s. This program utilized biological weapons simulants in open-air tests, including tests in New York subways, Washington D.C.'s National Airport, and the skies of San Francisco. The purpose was to mimic dispersal of the lethal originals with a substance the army claimed was completely safe in order to determine whether United States cities were vulnerable to biological attack. The simulants were either microorganisms (fungi or bacteria) or nonorganic substances such as zinc cadmium sulfide. Cole argues cogently that the simulants were not completely safe for everyone who was exposed in these often indiscriminate experiments. People who are already ill enough to be hospitalized, the elderly, and infants, in particular, are vulnerable to the bacteria. There were warnings of possible danger in the medical literature even at the time. Moreover, Cole marshals convincing evidence indicating that the tests over populated areas were not necessary. Everything "discovered" was either self-evident or obtainable through other, safer means.

For the account of the testing program and as an introduction to biological weapons and warfare, *Clouds of Secrecy* is worth reading. It is marred, however, by tendency to utilize hyperbole and to include material without explaining how it contributes to the central theme. There is, for example, a chapter on Gruinard Island, an uninhabited island off Scotland contaminated with anthrax spores by Britain and the United States during World War II (pp. 23–31). Although the island is clearly highly toxic to this day, the tests did not involve a populated site and did not utilize simulants. Cole does not show how the island's past and present fit in with the main subject of the book, simulant testing in populated areas.

More importantly, Cole's argument that the army has again begun to use biological simulants in open-air testing is less convincing than the historical analysis. His primary piece of evidence is a report by the Defense Department to the House Appropriations Committee in which he says they have admitted the practice. Cole includes extended excerpts from this report as appendix 4 (pp. 175–80). The excerpts do show that the army has used bacteria in testing protective or detection equipment. They do not, however, demonstrate that tests with the simulants were *recent*, that is, after the secret open-air tests were suspended in the late 1960s. (The text reads that simulants "have been used since 1943" [p. 176].) More importantly, while there is an explicit reference in the report to recent field testing of protective suits, presumably in the open air, it is not at all clear from the text whether what was being evaluated was the soldiers' performance *facing the simulants* in a protective suit, or the soldiers' ability to perform at all in the protective gear. One reference (p. 177) appears to be the key to Cole's charge: "The protective posture itself degrades fighting capability and this effect gets worse with time. The excessive loss of time and capability places U.S. forces at an extreme disadvantage, especially in the rapid and extremely lethal battles we now face. These effects have recently been evaluated in a series of field trials. The overall result is losing 30–50% of the fighting capability."

Although it is possible that simulants were used in the test cited, the text itself, as excerpted by Cole, simply does not give us enough information for a definitive conclusion. It is possible that the full text would do so. As it stands, the report is not "the first public admission that outdoor testing is now taking place with these bacteria" (p. 175).

The army categorically denies that any open-air testing with biological weapons simulants is now taking place, although they have applied for permission and facilities to do open-air testing at the Dugway Proving Grounds in Utah at some time in the future. (The environmental impact statement was completed in April 1989; hearings were scheduled in May 1989.) The proposed project is on a much smaller scale than the program of the

1950s and 1960s and if approved, will, according to Department of Defense sources, be conducted under much tighter controls. However, whether testing is now underway or not, Cole's general political and ethical point is correct. We need to consider carefully whether it should be the policy of the U.S. government to authorize the release—in an open-air setting less than one hundred miles from Salt Lake City—of bacteria and other simulants that have any potential to cause harm, however carefully the project is monitored.

FRANCES V. HARBOUR

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A Mob Intent on Death: The NAACP and the Arkansas Riot Cases. By Richard C. Cortner. Middletown, CT: Wesleyan University Press, 1988. 278p. $27.95.

Beginning with Clement E. Vose's account of the NAACP's quest to end racially based housing discrimination, analysts have been intrigued with organizational use of the courts to create social change. Vose's study moved a generation of scholars into this line of inquiry, a line that continues to flourish into the late 1980s.

As with most fields of study, scholars now approach group litigation in diverse ways. Some paint broad pictures, exploring the range of litigants and their strategies and tactics; others follow in the tradition of Vose, fascinating us with stories of group involvement in specific cases. Of this latter group, Richard C. Cortner stands as a leader. Over the past two decades, he has reconstructed, in a most detailed and thorough manner, such litigation quests as the ACLU's to gain judicial recognition of symbolic speech, the Women's Rights Project's to attain gender-based equality, and that of a coalition of more locally based groups to attack legislative apportionment.

Though Cortner's usual modus operandi is the case study, he never loses sight of the more immediate objective of understanding the total phenomenon of group litigation. Indeed, many of our conceptual constructs have evolved from Cortner's contextual examinations. Among the most important of these may be the "disadvantaged thesis," holding that groups unable to secure goals in the more political processes and institutions often turn to the courts for redress. In short, Cortner's attention to important detail, his meticulous use and documentation of sources, and his elegant writing style have brought many a litigation campaign to life, making the complexities of group litigation comprehensible.

In this spirit *A Mob Intent on Death* does not disappoint. Like Cortner's previous works, it is a highly readable and interesting account of the NAACP's litigation campaign in *Moore* v. *Dempsey* (1923), a landmark Supreme Court decision having major implications for the conduct of state criminal trials. And, like Cortner's past efforts, the book is a careful blend of the complicated set of historical events triggering the case, the legal factors at work during the suit, and those affecting the NAACP's participation. The story itself is told in painstaking detail, all of which is carefully documented with heretofore unmined archival sources and secondary literature.

In short, *A Mob Intent on Death* stands on its own as an excellent account of a most significant constitutional case. As usual, though, Cortner sets his sights beyond the particular contribution of *Moore,* bringing to light important aspects of the phenomenon of group litigation and providing scholars with fodder for future research endeavors.

Among the most important of these involves the capacity of organizations to undertake litigation, with a particular focus on the NAACP. It has often been the case that scholars point to the NAACP as *the* model litigating group. Fueled by stories of its prowess in the restrictive covenant and school desegregation cases, we have something of an awe-inspired reverance for its ability to etch its policy views into law. In fact, we base much of our understanding of litigation success and of the factors contributing to it on the activities of the NAACP and the NAACP Legal Defense Fund. In *A Mob Intent on Death,* we see a slightly different NAACP, a young organization struggling to overcome some basic weaknesses, namely, an uneven pool of legal talent at the trial court level, financial difficulties, and periodic leadership voids. That it was able to win the case despite these problems is significant. But the growth process it underwent during the course of this litigation may be equally as important. In this vein, Cortner's message is a clear one: "The NAACP's participation . . . contributed to its growing fund of experience in the conduct of constitutional litigation as a means of attacking the injustice and discrimination practiced against blacks" (p. 192). This maturation, in Cortner's view, certainly contributed to its later successes and, ultimately, to its "vitality as an organization."

This is a most important finding, one that has clear implications for current research on organizations, in general. It speaks to the evolutionary process that many successful groups must undergo before they are fully able to realize their policy objectives. Put in these terms, the NAACP's tremendous victory in *Brown* v. *Board of Education* was in part the culmination of the learning process it underwent during *Moore.*

This and numerous other findings make *A Mob Intent on Death* a must reading for all scholars concerned with interest groups, the courts, racial injustice, and the development of law. It stands as a solid contribution to an important and enduring field of study.

LEE EPSTEIN

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Freedom at Risk: Secrecy, Censorship, and Repression in the 1980s. Edited by Richard O. Curry. Philadelphia: Temple University Press, 1988. 448p. $29.95.

Human Rights in the States: New Directions in Constitutional Policymaking. Edited by Stanley H. Friedelbaum. Westport, CT: Greenwood, 1988. 345p. $39.95.

When U.S. historian Richard O. Curry was a Fulbright lecturer in New Zealand in 1981, he had an Orwellian encounter with the International Communications Agency (ICA) of the U.S. government. Curry had been invited to Australia to give a few lectures, but the ICA wanted to ask him some questions first. How, they wondered, did his topics fit into "the Australian 'game plan' of the ICA"? Jokingly, he said that he wasn't planning to create an international incident talking about subjects like the American Civil War and Reconstruction but, if he did, that a friend of his had urged him to make it a big one. The ICA official snapped back in all seriousness, "If you do, you'll never get another Fulbright" (pp. 179-80).

If this had been an isolated conversation, we probably would not have learned about it, nor would *Freedom at Risk* have been compiled. But Curry discovered that his situation was not unique and that other U.S. scholars were being systematically harrassed by Reagan administration agencies bent on institutionalizing secrecy, censorship, and repression, preventing the free flow of information and using knowledge in its war to win the hearts and minds of the U.S. people. *Freedom at Risk* documents these and other attacks on the civil liberties of U.S. citizens carried out during the Reagan years.

The parade of horribles is long, according to the unusual group of historians, lawyers, law professors, journalists, writers, activists, and miscellaneous academics represented in the Curry volume. The 25 essays and excellent introduction argue that the Reagan administration pushed its domestic and foreign policy agendas by suppressing information it found offensive or dangerous and spreading information it knew to be false. The administration severely restricted the ability of scholars to use the Freedom of Information Act, excluded from the country foreign nationals whose views were critical of administration policies, centralized the management of government information in the Office of Management and Budget, which then made the information less accessible by privatizing further collection, muzzled government officials who might speak to the public or the press, removed Watergate-era guarantees that the intelligence services would not pry without good reason into the private lives of citizens, spread viruses of international disinformation that then infected U.S. intelligence operations, encouraged widespread information gathering on ordinary citizens through drug testing and polygraphs—the list goes on and on. To consolidate its position, the Reagan administration filled the federal courts with judges who were likely to be hostile to the rights claims that the victims of these policies might have made at law.

The picture painted in the Curry anthology is bleak for those worried about the protection of civil liberties. The progress detailed in the Friedelbaum book is somewhat—but not a great deal—more hopeful on the point. If the federal courts are going to abdicate in protecting the rights of U.S. citizens, perhaps state courts can take the field. (Though for many of the issues that the Curry book discusses, it's difficult to see how they could come to be litigated in state courts.)

Human Rights in the States brings together eight essays, each of which reviews a different area of federal constitutional doctrine and explores the extent to which states have pushed rights protection beyond the level currently established by the U.S. Supreme Court. The record of state courts is mixed, both by topic and geography. State supreme courts have moved more in the areas of economic due process and free speech, less in the areas of privacy and criminal procedure. And overwhelmingly, whatever happens happens in New Jersey. California, New York, Massachusetts, and Washington have selected activism in selected areas. Virginia, Utah, Missouri, and Iowa have hardly any independent constitutional innovation at all. But New Jersey's Supreme Court clearly leads the way in developing an independent constitutional jurisprudence in many areas. Though state courts do sometimes in some areas provide innovative constitutional arguments, *Human Rights in the States* shows—inadvertently perhaps—just how little they have done.

Both the Curry and Friedelbaum anthologies are primarily descriptive works, detailing the state of affairs at the end of the Reagan years in their respective areas. Each book works better as a reference tool to be consulted on specific issues than as a book to read straight through. The Curry volume covers a vast array of topics, ably summarized in the detailed and thoughtful introduction. Readers interested in particular areas can go straight to the articles on their subjects of interest from there, bypassing the sometimes repetitive detail in the various essays. The Friedelbaum book covers the first amendment, gender equality, privacy, entitlement programs, criminal procedure, economic due process, citizen initiatives, and judicial federalism. The essays in the Friedelbaum collection are very similar in style and organization, summarizing decision after decision by state courts in each of a variety of issue areas, so readers may find it helpful to go directly to the issue that concerns them while missing out the others, even those covered in the same chapter. The pieces in the Curry volume add up to more than the essays in the Friedelbaum volume do, however, because the central theme of secrecy and censorship is drawn through all the essays, and the cumulative effect of learning about abuse after abuse of the Reagan administration is powerful (though the very bulk of the collection can be numbing). The Friedelbaum volume, trying to summarize what 50 state courts are doing in a myriad of different issue areas, shows the effects of an inevitable centrifugal force.

Freedom at Risk is a book for a general audience; and its essays, some of which have been published elsewhere in scholarly journals as well as more popular sources, are aimed at the average, educated reader. *Human Rights in the States*, with its specially commissioned essays, is written for the more scholarly audience that is broadly familiar with federal constitutional doctrine but wants to know what states have done for us lately. Both books provide quick access to information that is otherwise hard to track down but well worth knowing. But more scholarly investigation on these topics will be necessary to make more coherent narrative sense of the bits we see displayed here.

KIM LANE SCHEPPELE

University of Michigan

The Primetime Presidency of Ronald Reagan: The Era of the Television Presidency. By Robert E. Denton, Jr. New York: Praeger, 1988. 128p. $29.95.

Presidential Debates: The Challenge of Creating an Informed Electorate. By Kathleen Hall Jamieson and David S. Birdsell. New York: Oxford University Press, 1988. 272p. $19.95.

Consider the plight of presidential candidates or presidents who seek the public's support and understanding. No eloquence or strategy can be of any use to them without media to carry their appeals to their audience. Does it matter which media carry these appeals? Of course it does. Media of communication constrain both the form and content of communications. Should we have expected the advent of television to change what politicians do and say? Certainly, although to discover just how it has done so takes one into a tangled realm of complex effects, multiple causes, and unexplored potentialities. Both *The Primetime Presidency of Ronald Reagan* by Robert E. Denton, Jr. and *Presidential Debates* by Kathleen Hall Jamieson and David S. Birdsell are efforts to isolate some of television's effects on political communication, actual and potential, to identify the sources of such effects, and to evaluate them. *Presidential Debates* also suggests a way to do something about the problems it identifies.

Television's impact on the presidency is the central concern of Denton's book. Its argument goes as follows. Since the 1970s voters have turned to television as their primary source of information about presidents, presidential candidates, and the issues of government. Concurrently, presidential candidates and presidents have surrounded themselves with media consultants, the better to ensure television's favorable presentation of themselves and of their ideas and actions. Ronald Reagan became the nation's "first true television president" (p. xii), in part because he was adept in manipulating that medium to his advantage, but (more profoundly) because he conformed "to the essential needs and characteristics of television as a medium" (p. 11). His "laid-back style," sense of drama, gift for expressing how people felt, and cheerleading for an idealized image of his country were "right" for television (esp. pp. 64–77). Television and the television presidency have altered the quality of democratic government. Although the medium is "highly involving, it does not encourage critical involvement of information" (p. 57). Our future leaders will be "sports figures, movie stars, television celebrities, and political celebrities" (p. 83). In presenting this argument Denton touches on a wide range of related topics: the social and political power of the mass media, changes in political rhetoric, political campaigns, presidential primaries, and the special properties of television.

Presidential Debates is a book about political campaigning that focuses its attention on televised debates between presidential candidates. It asks whether such debates "provide voters with what they need to know to choose a president" (p. 6) and finds debates to be an imperfect but powerful means to inform. The book also argues that we should conceive of presidential campaigns as "a series of distinct but equally important communicative events" and proposes instituting a set of such events, some debatelike and some not, that would "increase the electorate's understanding of the issues, the candidates, and the campaign" (p. 199). More than other books about presidential debates, this one examines the history of debating in U.S. education and civic life and does so in a way that helps one to appreciate both the contributions and limitations of debates as a form of political discourse.

Both *Presidential Debates* and *The Primetime Presidency* are highly critical of contemporary political discussion and itemize its shortcomings at considerable length. It is much easier to criticize the quality of political discourse, however, than it is to suggest sensible standards for evaluating it. Political discourse is an instrument in the competition for power. If we take that fact as given, how good can we reasonably expect it be? Both books would be better if they had an explicit answer to this question, but neither does. Jamieson and Birdsell's proposals, setting nonutopian goals, at least constitute a plausible implicit answer. Denton has an implicit answer also, but it is a dubious one. He contrasts a sorry present with a golden past. No more, he writes, do electioneering politicians "try to convert through argumentation" (p. 36); no more do political messages "take the time to justify and explain" (p. 42); and "gone are the true and mature laborers, businessmen, educators and professionals that once comprised state and national legislatures" (p. 83). Readers will rightly doubt that the differences between past and present are quite so stark.

There is a stark difference, however, in how these two books go about making their arguments. *Presidential Debates* speaks to the reader's doubts. To support its points about debates and other forms of campaigning, it cites many apt examples and makes good use of findings produced by systematic studies of the media. What *The Primetime Presidency of Ronald Reagan* has to say sometimes suggests interesting lines of speculation ("It is simply impossible to find a Reagan speech that will be studied as a piece of literature or as an exemplary example of human persuasion" [p. 2]), is often glib ("Thus, politics becomes an activity of style over substance, image over reality, melodrama over analysis, belief over knowing, awareness over understanding" [p. 24]), and occasionally wrong ("Historically scholars and politicians approached political communication as simply a task of the movement of information" [p. 42]), but is hardly ever well supported by evidence. Much of the book reads, in fact, as if illustrations, qualifications, clarifications, and evidence had been systematically edited out. *Presidential Debates* debates. *The Primetime Presidency* often seems to be striving for the arresting "sound bites" that it deplores.

STANLEY KELLEY, JR.

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Rainbow's End: Irish-Americans and the Dilemmas of Urban Machine Politics, 1840–1985. By Steven P. Erie. Berkeley: University of California Press, 1988. 374p. $27.50.

This book should be read and used by those who teach courses with sections on machine politics in the United States. Those who may simply want to know something about the subject should gain some familiarity with it. It surveys the history of machine politics in eight cities where Irish-Americans were numerous: New York, Philadelphia, Chicago, Boston, San Francisco, Pittsburgh, Jersey City, and Albany. It is obviously, then, a survey of much more than a small sample of machine politics. Erie draws on a broad literature and effectively challenges some going interpretations of the machines.

Despite these substantial virtues, the book also presents some problems. I had a hard time keeping track of what the book was about. True, Erie does say up front that the book is a "study of big-city machine politics as well as of ethnic politics" (p. 8). The difficulty is that it is a study of both. His attention and the reader's moves back and forth and on to some secondary but significant themes. My interpretation is that we have two books in one.

It seems that Erie's first love was the story of the Irish and their experience in machine politics. He tells that story from secondary sources, along with some government documents on immigration, city finances, and city government employment. Historians and the historically minded would have found this a useful summary but not an "original contribution" of much magnitude, hence not a book. It would not have stood alone.

Political scientists would have found the material perhaps "merely descriptive" and unsatisfying to their thirst for "theory." Erie does include and test some social science and political science interpretations of political machines, but this analysis is not enough to make a book. Perhaps the journals would have provided an outlet, but Erie's method of testing by use of secondary and historical sources would be unfamiliar there. It might be said that there is material for several long essays, which are indeed to be found here. But still no book. Erie solves the problem by combining his story of the Irish with a theoretical gloss into one book.

The story of the Irish and of the machines and bosses they produced, as it is told here, is one many will find interesting and occasionally surprising. But I question its adequacy at some points. Erie is right to set aside the romanticizing of Pat Moynihan. He does attempt to explain the swift entrance and success of the Irish in U.S. city politics relative to other immigrants by the fact that they came knowing the language, which is adequate, though weak. A deeper knowledge of the Irish past and culture would help, but Erie seems not to have studied much Irish history. Reference to Robert Kennedy, certainly a lesser authority, for a summary of English rule in Ireland does not suggest scholarship. But English rule is, of course, important. Despite its early brutality and later stupidity, it had brought a knowledge of English politics, law, and liberties to Ireland, an experience very different from that of many people from eastern and southern Europe. And Catholic Emancipation came in 1827. This experience, as well as the language, was part of the baggage; and much more should be made of it than has been so far.

Erie notes (with some regret, I think) that the Irish bosses were not working-class leaders. He suggests that the explanation is largely in the need the bosses felt to work with business interests and to accommodate to them. But these men were solid middle-class people, as were many of the rank-and-file members of their organizations. They were personally honest, faithful to their families, churchgoers, somewhat puritanical. Revolution and reform were not part of their view of life.

Erie notes also the relatively slow entrance of the Irish into the professions and into the ranks of business owners and enterprisers. He attributes some of this to the supply of lower-level jobs offered by the Irish-dominated machines, and there may be a considerable element of truth in this explanation. But explaining the ways of various immigrant groups in U.S. life is a complicated task. Differences in skills, opportunities, and above all attitudes enter into the problem. Some immigrant groups wanted land. Others were alert to trading and mercantile possibilities. Some may have stuck to the ports of entry through timidity, being either unwilling to venture further into the unknown or fearful of life on the rural frontiers where conditions were harder and more dangerous than in the more comfortable cities. Accounting for these things, including the behavior of the Irish, needs a more elaborate explanation than the supply of political jobs. Also, their success in the Catholic Church is no small achievement and should be counted into the story.

Turning from the Irish story to the analysis of machine politics, a major theme is the critical examination of the "rainbow theory" of machine politics. In this theory machines are seen as multiethnic, or as "rainbow coalitions." The rewards were jobs and services drawn from the municipal government for the benefit of the partners. By implication, anyway, further rewards for the partners were general economic improvement now and in the future. Erie challenges the theory at every point. He makes the case that the Irish did not share power, did not share benefits, and did not themselves improve their own general position as a group by taking low-paid machine jobs. His evidence is abundant.

Erie contributes a narrow-gauge theory to account for the fact that given apparently similar conditions, machines grew and persisted in some cities but not in others. He explains these differences and other differences in the life cycles of city machines by differences in city machines' and would-be city machines' relations to state politics. The protection or hostility of state governments were vital factors in the growth and decay of the machines, at times decisive. This "intergovernmental theory" and Erie's critique of the "rainbow theory" both belong in the standard literature.

THOMAS A. FLINN

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Redefining the Supreme Court's Role: A Theory of Managing the Federal Judicial Process. By Samuel Estreicher and John Sexton. New Haven: Yale University Press, 1986. 206p. $11.95 paper.

Samuel Estreicher and John Sexton, law professors at New York University, are the latest in a long series of experts to insist that something simply must be done about either the work load or the priorities of the Supreme Court. Similar arguments have been circulating at least since 1959, when a *Harvard Law Review* exchange between Henry Hart and Thurman Arnold disputed both the quality of the Court's recent output and any connection between quality and work load. A 1968 article by Philip Kurland in the *New York Times Magazine* revived proposals for change, but the greatest impetus to new plans was probably the appointment of Warren Burger as chief justice. His tenure saw at least three new schemes proposed and forgotten.

In 1972 the Burger-appointed Freund Commission proposed the creation of a National Court of Appeals whose primary task would be to screen petitions for review. This recommendation was abandoned in the face of vehement opposition. The defeat of the next two proposals had less to do with attack than with governmental inertia. The Hruska Commission suggested a tribunal to which the Supreme Court could refer cases. In 1983, Senators Robert Dole, Howell Heflin, and Strom Thurmond introduced a bill establishing an Intercircuit Tribunal. Although Burger is no longer chief justice, his interest in judicial reform appears to have put this issue permanently on the national agenda.

As the book's title implies, Estreicher and Sexton find the roots of the problem not in the structure of the system but in the Court's conception of its role. The justices are too generous in granting review. These errors stem from the Court's mistaking its task as to consider "whether the Justices agreed or disagreed with the result" (p. 5), "to correct error in all federal cases" (p. 128), or "to ensure justice in the particular case" (pp. 135–36). These ideas are vestiges of a "nineteenth-century model" (p. 49). The authors propose "a managerial model of the Supreme Court's responsibilities" (p. 4), by which the Court would grant review only where there is "some structural basis for suspending the strong presumption of regularity that ordinarily should attach to the decisions of subordinate actors in the judicial system" (p. 5). The authors nowhere find it necessary to defend this presumption. The recommended criteria for case selection emphasize intercircuit, interbranch, intergovernmental, and interstate conflicts.

It would be unfair to describe this book as presenting the Douglas Brackman approach to the Supreme Court's function. But it is likely that were the Court to exercise its jurisdiction as these authors recommend, the rebarbative managing partner of *L.A. Law* would be more at home there than would anyone who sought what one unsuccessful nominee called "an intellectual feast." The authors have rejected such concepts of the Court's role as "teachers in a vital national seminar" (E. V. Rostow), expounders of "neutral principles" (Herbert Wechsler), protectors of minority rights (Hugo Black and Earl Warren) or "an intermediate body between the people and the legislature" (*Federalist* 78). Any vision so pedestrian as the authors' managerial model bears a heavy burden of justification. That burden is not met.

First, reformers must convince their audience that a problem exists. There is by no means universal agreement on this point. Several present and former justices have denied that the Court is overworked. In the case of petition screening, a frequent focus of reform proposals, some clerks have claimed that they learn in their first weeks on the job to perform this task fairly quickly. Even journalists who cover the Court have made similar claims. No student of the Court who was unconvinced of the need for change before reading this book will find compelling new evidence or argument here.

Still less consensus exists on the quality of the Court's output. Separating criticism of reasoning from criticism of result has always been difficult. This problem leads to the second task reformers face. They must convince their audience that their recommendations are free of political bias. It is not necessarily a coincidence that the proposals for reform have originated fairly consistently from the segment of the political spectrum that runs from the center to the extreme right. Even in the 1980s, as the Court moves to the right, the Left does not take up the cry for reform. The fact that the authors identify themselves as former clerks for, respectively, Lewis Powell and Warren Burger hardly allays the reader's wariness.

The extent to which *Redefining the Supreme Court's Role* fails to meet this second burden is revealed by the authors' casual and gratuitous reference to "the bleeding plaintiff model" (p. 129). The cloaking of ideological positions behind an ostensibly neutral administrative stance has become all too familiar in recent years, and it has characterized the judicial reform movement from its inception. The persistent failure of these proposals may indicate both lack of commitment to the political positions associated with these recommendations and a preference for the conceptions of the judicial role that these authors reject. Perhaps we have not yet become a nation of ideologues masquerading as managers.

JUDITH A. BAER

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Signals from the Hill: Congressional Oversight and the Challenge of Social Regulation. By Christopher H. Foreman, Jr. New Haven: Yale University Press, 1988. 214p. $27.50.

Whatever new ideas or findings this book may contain, they are presented in a curiously autistic style. If it is anything, the book is a contribution to the lively, conspicuous, and wide-ranging current literature on congressional control of the bureaucracy. Although aware that he is not the only one to have discussed this topic, Foreman crudely rewrites some of the literature without mentioning much of it.

Political scientists and public law scholars had long contended that Congress had abdicated a great deal of its Article 1 power to the bureaucracy by dint of delegation and the neglect of oversight. A more recent literature, sometimes associated with the label *congressional dominance*, has taken the opposite line. Delegation is not abdication so long as delegates are properly disciplined agents. As the creatures and wards of Congress, executive agencies would find it hard to pursue policies at odds with congressional goals. Delegation to a large bureaucracy often enhances congressional opportunities to intervene in government activity. Changes in bureaucratic policy often track changes in relevant congressional preferences in predictable ways. By using decentralized fire alarm oversight more than centralized police patrol oversight, Congress has instituted an elaborate and sophisticated system of checks and balances that makes bureaucratic shirking easy to correct—sometimes by congressional intervention, more often by administrative and judicial procedure. The entire mechanism is driven by reelection incentives and is associated with an emphasis on particularistic benefits. This line of argument has been supported by a large variety of empirical and theoretical contributions, and a satisfactory summary would require a long review of the literature.

Enter Foreman. Although sometimes irritating, reinvention of the wheel is not necessarily bad. New wheels sometimes work better than old, and extra wheels can support heavier loads. But Foreman's book offers little more than a flat tire. Closer attention to the literature would have prevented some critical misunderstandings that seriously impair his findings.

Foreman has three central arguments. The first, which he shares with the congressional dominance literature, is that the "runaway bureaucracy" rhetoric is false and that the common perception of regulatory discretion is inflated. Foreman claims that congressional oversight "works largely by appearing to have failed" (p. 6), yet he offers no argument or evidence as to how the appearance of failure leads to oversight success. Further, though many other scholars have sought to substantiate this argument with evidence linking changes in congressional preferences to changes in agency decisions, Foreman relies on anecdotal evidence about signals from the Hill. That signals were often sent and that they had some effect, however, does not address the issues of runaway bureaucracy. Congress can only signal an agency when it has the information on which to base the signal and the desire, or consensus, to send one. Though Foreman recognizes these problems, he does not address them in his evidence or argument. Thus, the stories he relates are unlikely to impress those who do not already share his conclusions.

Foreman's second argument is given largely without any attempt at substantiation. His claim is that congressional oversight is "means-focused." He argues that this leads to a failure to achieve the purposes of social regulation. A basic tenet of theories of institutional design and organization is that structure determines policy. The structure of the delegation (in this case the procedures that regulate decision making by executive agencies), to the extent that they are well specified, determine the outcomes. Thus, a basic result of the congressional dominance literature concludes that overseeing of the "means" of decision making in fact ensures that the goals of the regulation will be reasonably well met.

Third, he argues that political uncertainty, technical complexity, and political opposition make it difficult to implement social regulation. He recommends that reform of congressional oversight should be realistic, incremental, and sensitive to its effect on executive agencies and should not fall victim to the "runaway bureaucracy" rhetoric. Though his conclusions are well worn and his recommendations seem innocuous, Foreman never explains why we should follow the recommendations or why they would mitigate the problem of regulatory oversight (namely, "means-focusing") that he identifies. Nonincremental changes in oversight capabilities, as, for example, in the 1946 Administrative Procedures Act, have been very effective. Moreover, if the "runaway bureaucracy" rhetoric is electorally driven, it may be necessary for some political entrepreneur to whip such rhetoric up to stimulate the public, and therefore their representatives, to correct Congress's alleged failures.

In the final analysis, Foreman presents some nice anecdotes about congressional signalling, many of which are useful additions to congressional lore. What is right about his three central arguments, however, is not new; and what is new is not convincing.

MATHEW D. MCCUBBINS

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One Billion Dollars of Influence: The Direct Marketing of Politics. By R. Kenneth Godwin. Chatham, NJ: Chatham House, 1988. 186p. $12.95 paper.

The Best Congress Money Can Buy. By Philip M. Stern. New York: Pantheon, 1988. 196p. $18.95.

Money in U.S. politics—who raises it, how it is raised, and to what end it is used—is an issue of intense scholarly and popular interest. Driven by technological and statutory innovations, the financing of U.S. electoral politics has undergone tremendous change in recent decades. Although different in style, focus, and overall conclusion, these two books represent complementary additions to the growing literature concerned with this topic.

As its title suggests, Stern's book presents a strong indictment against the current method of financing congressional elections, particularly the role of political actions committees (PACs). Written in a muckraking style, the book is a call to action against a campaign finance system in which a substantial and growing portion of the dollars raised for congressional campaigns come from PACs, are raised by incumbants, and (in the author's view) are linked to policy decisions favoring the special interests represented by the PACs. The author's approach is journalistic, with particular emphasis placed on case studies of policy making and interviews with current and former members of Congress and PAC managers. The book is clearly intended for a general audience, although it should be of interest to readers interested in U.S. politics and to scholars in the area.

In contrast to the broad strokes of the Stern book, Godwin employs a more familiar social-scientific approach to the study of one component of the new system of political finance, direct marketing. The book blends a review of the existing literature with original research and ranges from a straightforward description of the mechanics of direct marketing (by mail, phone, and television) to evaluation of various hypotheses concerning the effects of direct marketing on citizen participation, interest group behavior, political parties, and policy formation. Unlike Stern, Godwin reaches mixed conclusions concerning the effects of this new technology. For example, in regard to citizen participation, he finds that direct marketing has neither enfranchised the lower classes (as proponents have sometimes predicted) nor created a society composed of either intolerant zealots or passive "checkbook participants" (as critics have argued). His most arresting—and I think questionable—conclusions relate to the effects of direct marketing on the Democratic and Republican parties and on congressional policy making. Essentially, he argues that the vastly superior direct marketing fund-raising efforts of the Republican party have interacted with the much weaker Democratic party efforts so as to "fragment the Democratic party and encourag[e] candidate-centered campaign organizations where candidates were forced to fend for themselves" (p. 148). As a result, Democratic candidates have become much more dependent on business and trade association PACs, and Democratic members of Congress have correspondingly altered their roll call voting behavior to reflect this new dependence on PAC dollars.

Both books reach a conclusion that the public interest is currently at risk. Although Godwin's conclusions are more reflective of the mixed evidence presented by recent scholarly research, both authors conclude that congressional decision making is being adversely affected by PAC contributions and propose reforms to the present system. As might be expected, Stern's proposal is more radical—a comprehensive public financing system for congressional elections directly modeled on the current presidential system. Godwin makes more modest suggestions designed to decrease the proportion of funds coming from PACs promoting economic interests, as well as changes in reporting and enforcement procedures designed to make PACs more accountable to both the public and their own members.

Neither book is without its limitations. Aside from the extensive quotations, the Stern book presents little that is not available elsewhere. Readers wanting basic information about PACs would be better directed to a book like Sabato's *PAC Power* (1985), a major source for Stern's overview chapter, "A PAC Primer." In addition, Stern's enumeration of correlations between receipt of PAC contributions and favorable treatment of the PACs' interests by the members of Congress does not represent a demonstration of an *independent* effect of these contributions on behavior (the specific question of interest to scholars). Godwin's book, too, suffers somewhat by comparison to Sabato's when the author moves from the original topic of direct marketing to the broader subject of PAC influence. For example, on the crucial question of the relationship between PAC contributions and congressional behavior, Godwin relies on three studies to make a case for a clear connection. By contrast, Sabato bases his discussion on 14 separate studies (including the 3 relied on by Godwin) and reaches the less-alarming conclusion that instances of linkage between contributions and roll call voting are very much the exception.

Despite these limitations, each of these books represents a valuable addition to the literature in this area. The Stern book is an important counterweight to the growing body of research that finds little evidence to support the claim that PAC contributions influence congressional decision making. It is impossible to read this book without getting a sense of the tawdry, sordid side of the current system of fund-raising. Just as Fenno's *Home Style* (1978) employed a qualitative approach to bring one aspect of members' behavior alive, Stern's reporting of the views of elite participants, combined with accounts of behaviors not often measured by more quantitative techniques, paints a picture that the scholarly community needs to see. Godwin's book is important for two reasons. First, he carefully examines several questions that relate specifically to the subject of direct marketing and in doing so advances the important goal of untangling and evaluating the various elements of the new system of political finance (like PACs, direct marketing, negative advertising, and incumbency advantages) that so often are all grouped together as part of the "money-in-politics" problem. Second, he identifies secondary as well as primary effects of this new direct marketing technology. There is more than one step that leads from the direct marketing effort of the 1964 Goldwater campaign to the voting behavior of Democratic legislators in the 1980s, and Godwin traces each link in the chain. Taken together, these quite different books will give interested readers much to think about.

JOHN P. FRENDREIS

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The Social Meaning of Civic Space: Studying Political Authority through Architecture. By Charles T. Goodsell. Lawrence: University Press of Kansas, 1988. 254p. $25.00.

The buildings and architectural settings within which governmental and political actions take place seem to have a perennial fascination for those who study such events. The institution of the presidency, for example, is referred to in terms of the building that houses the most conspicuous members of it, the White House. Similar habits prevail in other countries; but to stay in the U.S. context, surely one of the most familiar examples of this tendency to identify government with its architectural location is the widespread practice of referring to the local municipal government as "city hall."

Despite this, political scientists have not given much direct attention to exploring the possibility of there being significant relationships between government and its buildings that merit sustained, disciplined attention. Goodsell's book is a ground-breaking effort to show that such efforts can and should be made. Its focus is the architecture of U.S. city halls, and its purpose is to show the connection between a government's conception of itself and its citizens on the one hand and the space that it organizes for the interaction of the two on the other. This is an appropriate task for a political scientist, as opposed to an art historian or an architectural critic because political science uniquely addresses the study of governmental practices and the claims made to justify them. This sort of study requires being able to use the intellectual tools of art historians and architects. It also requires making use of their insights to interpret the subjective responses of both governors and the governed to the architectural settings where they encounter each other.

Goodsell set himself the task of acquiring a working knowledge of matters that are normally unfamiliar to political scientists. These efforts are outlined in the preface and first two chapters, and they should serve as a discouragement to anyone who might be tempted to try a quick replication or imitation of this study. To carry out his project, Goodsell surveyed 75 city council chambers around the United States. The survey included large, medium, and small cities. It covered all sections of the country. Paralleling the growth of

U.S. cities, chambers were also selected from three time periods: 1865–1920, 1920–1960, and since 1960. The council chambers were measured and photographed; the style and configuration of the spaces were described, as were decorations, embellishments, souveniers of the municipalities' pasts, and technological innovations.

The results of this survey make fascinating reading for anyone with a taste for the topic, and an appendix contains some helpful suggestions for municipal officials who may be planning to build new council chambers or to renovate existing ones. But the major point of the book extends well beyond the particular concerns of those who happen to work with municipal governing bodies. The detailed study of the council chambers provides the empirical base for a larger argument that involves central questions about political symbolism and political culture. Goodsell persuasively makes the case that governmental architecture and the uses to which it is put offer valuable evidence of the ideas and values at work in a given regime. The study of political symbols cannot be confined to verbal symbols or simple signs that "stand for" larger entities; it must also include the spatial context within which people experience meaning. One of the implications of this finding is that political studies should demonstrate a greater awareness of the concrete and experiential aspects of political activity.

As is the case with any pioneering work, there are areas where it would have been helpful to have both more explanation and more research available. In assessing the meaning of various chambers Goodsell seems to rely on a rather stark polar opposition of democratic and authoritarian values in some places; yet more complex senses of the relation of governors and governed appear in other parts of the text. And though Goodsell mentions the importance of "ritual," his assessment of the chambers relies on their architecture rather than on how they are used.

Still, taken as a whole, Goodsell's book is fresh and stimulating. It should be regarded as a required acquisition for any library or personal collection that aims at coverage of urban government, U.S. political culture, or the study of political symbolism.

JOSEPH F. FREEMAN

Lynchburg College

The Life and Death of the Solid South. By Dewey W. Grantham. Lexington: University Press of Kentucky, 1988. 272p. $25.00.

The Two-Party South. By Alexander Lamis. Expanded edition. New York: Oxford University Press, 1988. 432p. $34.00.

It is not difficult to understand why political scientists, historians, and journalists are drawn to write about Southern political life to an extent unthinkable for other sections of the United States. Politics—like recreation, cuisine, and speech patterns—*is* different in the South, but, as these two books make abundantly clear, not as different as once was the case. The one-party system that set the South apart for so long is now certifiably dead. Although neither of these two books offers major new findings or interpretations, their nicely complementary postmortems, taken together, provide a useful guide to the origin, maintenance, and eventual demise of the one-party tradition.

The Grantham book will perhaps have more appeal because of the overview it provides. Grantham sketches clearly, succinctly, and authoritatively the coming of Democratic hegemony in the last quarter of the nineteenth century, its transformation into virtual monopoly in the first three decades of the twentieth century, and then the slow unraveling of the one-party system touched off in the 1930s. The author makes his way across the decades in a surefooted fashion, providing just enough detail to make his generalizations convincing. I do find puzzling his inclusion of Kentucky and Oklahoma as part of the solid South, but otherwise the opportunities for faultfinding are limited to an occasional careless aside. For example, it is not the case that the South was seldom represented on the Supreme Court from the Civil War to 1910 (p. 58); and the assertion that Estes Kefauver was the first Southerner since Woodrow Wilson to undertake a "genuine national campaign" for the presidential nomination (p. 127) overlooks William Gibbs McAdoo, every bit as much a Southerner as Wilson.

Resourceful reviewers, of course, can always complain about what is not said. Even a brief discussion of party history in the antebellum South might have helped to make more understandable politics in the defeated states during Reconstruction. And though Grantham identifies the South's political culture as one of three key forces creating the solid South (the other two being the Southern economy and the federal system), he does little to explain and apply the concept.

Offsetting those sins of omission is Grantham's success in explaining the long-term impact of the New Deal on politics in the South. Judged in terms of votes garnered and elections won, the Southern one-party system appeared to peak in the 1930s. But the New Deal generated tensions both within the South and between the then-dominant forces in the South and those in the rest of the country that systematically undermined the Democratic party's hold on the region.

Alexander Lamis's *The Two-Party South* expands the 1984 edition by adding three chapters dealing with the 1984 and 1986 elections plus a brief concluding overview, leaving the text of the earlier version unchanged except for updated computations of the composite measure of Democratic party strength. Whether that is the most desirable way to revise a book is debatable, but it has the virtue of facilitating comparisons of revised generalizations with earlier ones.

Lamis is most successful in providing an orderly and succinct account of electoral events in each former Confederate state over the past three decades—who the candidates were, the major issues, and the vote totals and distributions. Because the chapters devoted to particular states average only 14–15 pages each, the author necessarily has to paint with a very broad brush, concentrating most tables and figures in the general chapters, especially chapters 3 and 16. That Lamis is generally successful in his reporting can be attributed to his own extensive knowledge of the subject and to familiarity with a wide variety of scholarly and journalistic sources (the latter too much in evidence at times).

On the whole, Lamis does a commendable job of keeping facts straight, though inveterate quibblers will find an occasional tidbit. (By the 1920s, the GOP had only three decades of national dominance, not the seven asserted on p. 21; contrary to the aside on p. 103, Texas liberal Democrats could not have been disgusted with the conservative nominee of their party in the 1961 special Senate election that brought John Tower to the Senate, because candidates were not nominated by parties.)

Lamis's broadly explanatory efforts are keyed to two themes: the salience of economic issues and the extent to which a black-white coalition has been achieved within the Democratic party. He finds economic issues to be less evident in the Deep South (Mississippi, Alabama, Georgia, and South Carolina) than in the Rim South. By contrast, Democratic success in developing a stable black-white electoral partnership cuts across the Deep South–Rim South division. Significantly, Lamis refuses to incorporate into his analysis any consideration of the question of party realignment. In a brief justification of his refusal (chapter 18), he makes clear his conviction that the concept is not a very useful one, at least in trying to understand Southern politics over the past three decades.

With rare exceptions (Earl and Merle Black's *Politics and Society in the South* [1987] being the most notable), scholars seeking to explain Southern politics have generally found it necessary to resort to state-by-state analysis. Both of these books use that approach (Grantham to a lesser extent), thereby making clear that the differences *among* Southern states are often as great as the differences *between* Southern and non-Southern states.

CLIFTON McCLESKEY

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Jimmy Carter As President: Leadership and the Politics of the Public Good. By Erwin C. Hargrove. Baton Rouge: Louisiana State University Press, 1988. 264p. $24.95.

The Carter Implosion: Jimmy Carter and the Amateur Style of Diplomacy. By Donald S. Spencer. New York: Praeger, 1988. 176p. $38.95.

The Press and the Carter Presidency. By Mark J. Rozell. Boulder: Westview, 1988. 230p. $28.00.

Presidents take their place in U.S. history as each succeeding wave of scholars makes its new appraisal. Scholars are the new image makers when the final campaign is over and the press corps has moved on. With three books on the style, foreign policy, and press coverage of the Carter administration, it appears to be Jimmy Carter's turn. There is less material here, however, than might be expected from the combined number of pages. The argument and contribution of each book can be summarized briefly.

The Carter Implosion is a strong attack on the foreign policy of the Carter administration and what the author calls the amateur style of diplomacy. The president's noble goals could not be translated into reality, the author argues, because the Carter White House did not understand the context or the pursuit of diplomacy. There was an imbalance of means and ends. Details are provided on individual advisers and the conflicts among them. According to the author, the Carter administration was responsible for the Iranian Revolution, the hostage crisis, the loss of detente with the Soviet Union, and other embarrassments around the globe. Camp David and the Panama Canal treaties are not mentioned. The book is interesting for its insights on diplomatic history (Gibbon, Tocqueville, Czar Nicholas of Russia), but beyond praise for the Nixon-Kissinger diplomacy there is little attempt to place events in context of the immediately preceding years. The detail supplied is highly selective and the argument has been made before.

In *The Press and the Carter Presidency* Mark Rozell sets out to record, exhaustively, the negative press coverage Carter received by three major newspapers and three weekly magazines. The book organizes its quotations chronologically by chapter (year 1, year 2, year 3) and chronologically within the chapters (January-March, April-June). Short summaries at the end of each chapter provide further analysis. The author's thesis is that the early negative criticism of the Carter administration was continued, with only minor variations, through the four years. The press never lost its negative view. Comments by staff people suggest that they were aware of the negative treatment and could not change it. This point is interesting, given the amount of literature suggesting that the White House can influence press coverage and in its usual battle with the press achieves at least a draw. The quotations in themselves are persuasive, although we are not given a means of evaluating the press story as a whole or of comparing Carter's coverage to other presidents' coverage. The raw material provided in the book might be used by others for more extended analysis and interpretation.

Jimmy Carter As President by Erwin Hargrove is easily the most satisfying account of the three, consisting of Hargrove's personal appraisal of the Carter record, supplemented by oral histories supplied by White House personnel and recorded at the Miller Center of the University of Virginia. Carter's conception of leadership, according to the author, was to seek policy problems to solve—problems of the public good—and to let the political consequences follow. The president's willingness to take on daunting problems, relying on "centralized colleagiality" and ignoring political repercussions are the strengths and also the weaknesses from which Carter's record in domestic, economic, and foreign policy is reviewed. While many of Carter's difficulties were inherited from the past, there were problems with the advisory process, conflicts produced by attempted colleagiality, and trouble engendered by pursuing policies—with the Soviet Union or with the Senate—without regard for their political costs.

A final chapter gives the author's appraisal. Presidents must be judged against the historical circumstances. They can be skillful or inept under favorable conditions (situations 1 and 2) or skillful or inept under unfavorable conditions (situations 3 and 4). Hargrove comes out for a type 3, skillful under unfavorable conditions: "Many conditions were highly unfavorable to the exercise of leadership at all. . . . Most of the achievements were personal and it is not clear that anyone else could have done as well."

It is worth noting that Hargrove's portrait does not contradict the substance of Spencer's critique or the thrust of press criticism compiled by Rozell. All three books point out the president's high goals and political carelessness. Many of the same events are selected as examples. This is not surprising since Hargrove quotes the staff and Rozell quotes the press, and staff and press have heard each other's comments. None of the books compare Carter to other presidents. No one asks how such notions as political carelessness might be pinned down and measured. No one cites a literature on the presidency that suggests ways of evaluating political skill or breaking out of the circle of contemporary assumptions.

Each book has a noteworthy thesis that will be cited: Carter's basic problem-solving approach; the critique of foreign policy; the

White House's inability to override or manipulate press views. Hargrove's book will take its place in accounts of the various styles of presidents and ways of managing the White House. Still, none of these should be considered must reading in a field where new books seem to be published every day. Perhaps it is too early to seek an appraisal of the Carter record. Jimmy Carter's turn may be still some years away.

BARBARA HINCKLEY

New York University

Party Campaigning in the 1980s. By Paul S. Herrnson. Cambridge: Harvard University Press, 1988. 192p. $25.00.

The subject of this tidily organized little book is the role that our national parties (the two national committees and the four congressional committees) play in congressional elections.

As Herrnson makes clear, the national parties do not do anything at all by themselves. What they do is *help.* They help recruit candidates, manage campaigns, develop campaign issues, prepare mass media advertising, raise funds, and gauge public opinion. They do not help very much with candidate-targeted voter registration and get-out-the-vote drives, leaving those tasks largely to the candidates and the local parties. In every kind of assistance, the Republican committees do more than their Democratic counterparts; and both sets of committees do more for competitive candidates than for those who are sure to win or sure to lose and more for candidates for the House than for candidates for the Senate.

These broad generalizations, in themselves neither surprising nor unfamiliar, are nicely fleshed out with some hitherto-unknown details and interestingly embedded in an analytical framework that addresses (but never really answers) the key question, Is more electoral activity, better financed, all that is required for parties to be renewed?

According to Herrnson, we have two models we apply to U.S. political parties: the original model of old-fashioned political machines and the later model (in the 1960s) of peripheral organizations in accelerating decline. He finds no fault with the pre-sixties model despite David Mayhew's clear demonstration (in *Placing Parties in American Politics* [1986]) that such machines existed in only eight states. Never mind. For Herrnson, the analytical problem lies in the later model. We need, he says, to recognize that the parties have assumed a *third* identity, that they are now powerful intermediaries, no longer peripheral to our national politics.

Others have noted the electoral resurgence of the national parties (see Larry Sabato's *The Party's Just Begun* [1987] and Xandra Kayden and Eddie Mahe, Jr.'s *The Party Goes On* [1985]). Herrnson's contribution is an impressive body of information regarding exactly how the six national committees have, collectively and individually, created that resurgence. Especially useful are his explanations of "agency agreements," the technique by which the national committees can transfer monies to state parties; the tactics of candidate recruitment used by the four congressional party committees; and the "conduit role" all six committees play in "bundling" and funneling PAC money to preferred candidates.

These contributions alone make *Party Campaigning in the 1980s* an important addition to the parties literature. Far less satisfactory, however, are the efforts of a concluding chapter to persuade us that a party capable of playing a stronger role in electoral politics may some day "be able to forge a nationwide consensus uniting a majority of voters behind a platform that party officials will implement after their election" (p. 128). To make this assertion, Herrnson must forget his own evidence that national party interest in candidate recruitment is based on electability, not programmatic compatibility; that in bundling PAC monies the parties strengthen, rather than weaken, candidate dependency on, and incumbent subservience to, special interests; that the practice of lending selective assistance to marginal candidacies is inconsistent with building, across the nation, the strong grass roots organizations that must be the first building block in any democratic model of responsible parties; and finally, that far from taking their lead from the national party committees (seemingly essential for enhanced party discipline), the congressional committees operate in near-total independence.

In sum, Herrnson has done a fine job of showing us that the parties are newly important as intermediaries. But they have become, his evidence would suggest, intermediaries not between citizen and state, but between PAC and candidates. That may be a good way to win elections, but it is not a good way to establish responsible party government. There is no point in confusing the one with the other, no matter how keen some may be to proclaim "party renewal" a fait accompli. If this be renewal, I'll take lingering death, any day.

KAY LAWSON

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The Irony of Regulatory Reform: The Deregulation of American Telecommunications. By Robert Britt Horwitz. New York: Oxford University Press, 1989. 424p. $39.95.

The author traces the history of the regulation and deregulation of U.S. telecommunications in the "postindustrial" society. This book is a valuable contribution to understanding U.S. society of the 1990s, best described in my perspective, as the cable news network society, a review of the world's latest developments in 30 minutes. The primary focus of this book is an analysis of broadcast and telephone industries. However, Horwitz also addresses regulation and deregulation of airlines, trucking, intercity buses, railroads, and banking.

Students of politics will be especially interested in how the forces of conservative, free market, economic theory and a left-liberal theory of political participation combined to bring on the deregulation of telecommunications. Therefore, two disparate U.S. ideologies drive telecommunication deregulation. Conservative, free market, economic theorists, grounded in laissez-faire ideology and perceiving regulatory agencies as vast bureaucracies whose arbitrary ways promoted economic inefficiency and artificial protectionism, called for reform of the FCC. Liberals and public interest groups, encompassing the rationale of participatory democracy and perceiving the FCC as captured by its clientele, advocated deregulation reforms to break up entrenched corporate power.

Consumer demands for sports, news, movie, porn, Christian, and Spanish-speaking television channels; the breakthroughs in telecommunications technology (with new services and delivery options of cable systems); the end of the half-century of monopoly of AT&T in telephony (with U.S. Sprint and MCI as alternatives); liberalization of entry into telephony and broadcasting industries (with entrepreneurs like the cable news network's Ted Turner entering the marketplace); and a romantic version of free speech (confronting a dynamic marketplace of ideas) led to a telecommunications revolution in U.S. society and revolutionized the bureaucracy and policies of the FCC.

This book is a well-crafted, important history of the politics, bureaucracy, and marketplace of telecommunications. The book is especially significant because telecommunication, as an infrastructure of modern society, is not only crucial for commerce but incorporates public boundaries of ideas and discussion, including freedom of speech. In exploring a distinctly modern political and philosophical paradox, that of telecommunications deregulation, Horwitz addresses how to guarantee meaningful freedom of speech in an age of information abundance. Subthemes focus on telephony and broadcasting. The author's key contribution is his analysis of the role of the state in a capitalist economy.

The author reviews theories of regulation, pointing out three phases of regulation (Progressive Era, New Deal, and Great Society); describes the development of telegraphy, telephony, and broadcasting prior to the establishment of the FCC (1934); explains the regulatory protection of AT&T; chronicles the period of extraordinary political activism and certain hostility to capital (1965–75); and traces a very nonlinear road to regulatory reform. This text serves well as a history of telecommunications and U.S. regulatory behaviors.

Horwitz emphasizes that the process of deregulation of telecommunications is unique in several ways. Technological innovation is an important factor not found in other industries. The actual deregulation of telecommunications focused on the bureaucratic politics within the Federal Communications Commission while deregulation of trucking, railroads, banking, and airlines occurred within the political context of Congress. The U.S. Department of Justice and a federal district judge arbitrated the breakup of AT&T's telephone monopoly.

Finally, the telecommunication infrastructure entails commerce and consumer equity but addresses issues of free speech as well. The consequences of deregulating telecommunications, therefore, are more complicated and ambiguous than other price-and-entry industries.

Despite the unique combination of efficiency and equality in revolutionizing this technocratic industry, the political economy of telecommunications remains the same. As Horwitz surmises, "Wealth buys media access and communication power, and the marketplace of ideas is overwhelmed by the wealthy. The First Amendment right of most citizens is a right to listen and watch. The free and open debate necessary to a democracy goes unfulfilled" (p. 282). In this view large, private, media interests may become private censors.

Therefore, greater diversity in the industry does not necessarily result in enhanced competition. Co-optation of minority and citizen groups by wealthy groups is an omnipresent concern. The transgression of media dollars on citizen rights jeopardizes a modern society dependent on information and communications and frustrates open entry and access to the marketplace of ideas. In the author's words, "Deregulation has brought competition and short-run economic efficiencies along with long-run uncertainties about stability, concentration, and discrimination" (p. 284).

An informative, thoroughly researched, and timely analysis of our cable news network society, this book contributes significantly to the literature of partisan, policy, and systems politics, enhancing our understanding of the ever-increasing complex relations between government (FCC bureaucracy) and business (telecommunications). The text's title, *The Irony of Regulatory Reform*, symbolically captures the identity and context of the telecommunications industry as the United States enters the 1990s.

JOHN E. ROUSE, JR.

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Ideological Budgeting: The Influence of Political Philosophy on Public Policy. By Steven G. Koven. New York: Praeger, 1988. 201p. $35.95.

The public budgeting literature of the last 20 years has been obsessed with two basic avenues of inquiry: debating whether budgeting is incremental or not and analyzing the relative merits, successes, and failures of major budget reforms. More recently, the political business cycle literature has provided a third avenue of inquiry, that of testing the impact of election cycles on fiscal policy outcomes.

These three areas of inquiry about how public financial allocations are made are important, but without additional theorizing they are mined with increasingly diminishing returns. Koven's book is instead a focus on the impact of ideology on budgeting outcomes and, as such, provides a rich backdrop to the drama of fiscal infighting. He raises the reader's consciousness that budget decisions are not impassionate and technical, made from mounds of ideologically meaningless data, but rather derive from fundamental worldviews about government and its role in the economy.

The first part of the book provides a lucid, understandable, well-written explanation of what ideology is and discusses specific examples. Chapter 1, the introduction, identifies traditional explanations of budgetary outcomes, including incrementalism, and discusses why these explanations in isolation are inadequate. Chapter 2 is a brief attempt to define ideology and to explore why operationalizing the concept is difficult. The ideologies of nationalism, communism, socialism, capitalism, fascism, and democracy are discussed in chapter 3.

Here, perhaps, a philosopher might quibble with Koven's choice to lump political ideologies, such as democracy and nationalism, together with economic philosophies, such as capitalism. Nor, if ideology is (as Koven defines it) an internally consistent worldview held by a large number of people, does Koven contend with whether these six basic ideologies are to be regarded as more or less mutually exclusive or not. But his skill at abstracting the important elements of these ideologies is considerable, and his style is highly readable.

Chapter 4 explores contemporary U.S. belief systems, including U.S. reform liberalism, neo-liberalism, the new Right, and neoconservatism. He addresses antecedents to U.S. liberalism, as well as its recent decline in salience.

The last three chapters explore the impact of ideology on budgeting in more specific detail. Chapter 5 uses limited data to examine this at all three levels of government—national, state, and local. At the national level, budget outlays as a percentage of GNP vary considerably across time when periods that correlate to overall ideological dominance are developed. At the state level, Americans for Democratic Action (ADA) scores for House and Senate members are used to measure ideology and are correlated with per capita total taxes and general expenditures. At the local level, two case studies—liberal New York City and conservative Dallas—are compared. In a more elaborate analysis, the 20 largest cities are rated ideologically using ADA scores, and ideologies are linked to budget composition.

Chapter 6 examines the potential, and often actual, conflict between rational and ideological budgeting, while chapter 7 discusses how ideological decisions affect policy judgments. Koven has written a highly readable, well-informed, and very thoughtful book on budgeting. If he is correct, the two-decade-long focus between incremental and rational budgeting needs to be reoriented instead to an examination of the tension between ideological and rational budgeting. The case he makes for this refocus is convincing. As such, his book is a very useful contribution to both the budgeting and public policy literatures and well worth reading.

MARCIA LYNN WHICKER

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The Limits of Judicial Power: The Supreme Court in American Politics. By William Lasser. Chapel Hill: University of North Carolina Press, 1989. 354p. $32.95 cloth, $10.95 paper.

There have been, arguably, five great periods of controversy over the nature and extent of judicial power under the Constitution. The first came when John Marshall ascended the bench in 1801 with a gleam in his Federalist eye toward making the national Court truly supreme. The second came with the infamous *Dred Scott* case, which plunged the nation into a bloody civil war. Following the aftermath of that fiery trial, the Court found itself once again in the midst of controversy over just how far Congress could go in reconstructing the shattered Union. Then came FDR and his attempt to cure politically what he saw as the constitutional ills of the Great Depression. And finally, in the mid-1950s, came the period in which we still live—the age of the new activism and the modern Supreme Court.

As William Lasser rightly points out, the decision of the Warren Court in *Brown* v. *Board of Education* "changed forever the role of the Supreme Court in American politics and society." With both insight and concision, Lasser demonstrates how the Warren Court developed into something of a judicial bull wreaking doctrinal havoc in the constitutional china shop. But cataloging the alleged abuses of judicial power in the modern era is not Lasser's point. Rather, he chooses to explain *politically* just how the Court is able to pull it off in the face of seemingly endless criticism and occasional shouts for the justices' heads.

Lasser's thesis is that history is the best clue as to the nature and extent as well as the evolution of judicial power generally and the role of the Supreme Court in particular. This he develops by focusing on three of the great crises that have surrounded the Supreme Court: the *Dred Scott* case, Reconstruction, and the New Deal. The lesson he draws is a simple one: "The Court entered the modern era not with a history of weakness, but of strength; not with a history of failure, but of success; not with reason to fear judicial activism, but with reason to assume the vitality of its own power. Simply put, the record of the modern era is not an aberration, but a continuation of a long-established historical pattern."

Yet (the author concedes) there is something different about the modern, post-Warren Court era. The fact is that the Court no longer seems plagued by catacylsmic moments (like *Dred Scott)* that rile a vociferous and active public response. Today (sadly, it seems) the public has come largely to acquiesce in the face of judicial intrusions into the most sensitive areas of public policy. "For the modern Supreme Court," Lasser notes, "a sort of low-level, permanent crisis has become routine." The inescapable conclusion for those bent on curbing the courts, in Lasser's estimation, is, Go ahead and try, but you won't have much luck. The reason, he argues, is that "the political constraints on the Supreme Court are indirect, inefficient, and uncertain."

Largely without polemical excrescences, Lasser's account of these matters is a competent, if not a compelling, one. The great contribution of the book is that it provides a much-needed historical context for the current debates over judicial activism. Lasser reminds us of our past in order to allow us to come to grips with the present and better to prepare for the future. His history is sound, his political sense sharp.

The defect of this book is that it approaches the subject within too narrow a compass. In particular, Lasser's use of Robert McCloskey's *The American Supreme Court* (1960) as both his point of departure and point of arrival buys focus at the expense of comprehensiveness. And the price is high. By focusing on so small and distant a target, Lasser loses the richness his topic is capable of producing. His further fascination with political scientists of yesteryear (Robert Dahl, Louis Hartz) more than with legal scholars of today results in a work that all too often reads more like a dissertation than a book.

But in the end that may not be so bad a thing. All too often the contemporary debate is carried on more with histrionics than with a sense of history. By rooting his argument in the soil of the scholars who first ploughed this ground, Lasser is able to escape an approach even more tedious than the one he has chosen, that of wading through the mountain of scholarly waste that has been generated by the contemporary arguments over the role of the Supreme Court in U.S. politics. With a jargon all its own, current legal scholarship has become largely impenetrable even to those who punish themselves by studying it. Lasser sidesteps that problem and thus has offered a book more readily accessible to a broader audience. For that alone, he is to be commended.

GARY L. MCDOWELL

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Contemporary Southern Politics. Edited by James F. Lea. Baton Rouge: Louisiana State University Press, 1988. 400p. $35.00.

The past decade has seen a resurgence of published research on southern politics. The publication of such influential books as William C. Havard's edited *The Changing Politics of the South* (1972), Numan V. Bartley and Hugh D. Graham's *Southern Politics and the Second Reconstruction* (1975), and Jack Bass and Walter DeVries's *The Transformation of Southern Politics* (1976) has been followed in the 1980s by similarly significant works by such scholars as Earl and Merle Black *(Politics and Society in the South* [1987]) and Alexander P. Lamis *(The Two-Party South* [1984]).

One of the most recent products of this new wave of southern politics research is James F. Lea's edited *Contemporary Southern Politics,* a collection of 13 essays addressing various elements of the South's current political system. The essays are linked, at least loosely, by their common concern with examining the dramatic changes that have occurred in the region since World War II, especially since the mid-1960s. This general theme places the volume squarely in the mainstream of current southern politics literature.

The topics analyzed range over a wide area: economic development, public opinion, black politics, the political party system, campaign style, and political institutions. Additionally, there is a chapter on Jimmy Carter's political career as a manifestation of southern political and social change and a summary essay utilizing Daniel Elazar's subcultural framework as a device for describing and evaluating recent change in the South. While there are some notable omissions (for example, the role of religion in southern politics and society is nowhere directly addressed), most of the usual subjects of interest to southern politics observers are explored. Indeed, the impressive scope of the topical coverage is one of the strong points of this volume. Of particular significance are the four essays examining the region's state legislatures, governorships, judiciaries, and bureaucracies. These constitute one of the major contributions of the book, inasmuch as they explore aspects of southern politics that are seldom included in general treatments of the subject and thereby help to fill a gap in the existing literature.

Such broad coverage, however, is not without its problems. The key drawback to this approach is that the individual chapters cannot possibly present the in-depth analysis found in more detailed treatments. For example, relegating discussion of changes in the southern political party system to a single chapter falls short of the much more extensive examination of the topic found in such works as *The Two-Party South* and *Politics and Society in the South.* Similarly, the excellent chapter on blacks in southern politics includes only a very brief consideration of Jesse Jackson's significance in southern politics over the past decade. As a final example, the concluding chapter presents a number of interesting and thought-provoking observations about subcultural change in the South, but the understandable space limitations prevent a full development of these points complete with the inclusion of supporting data. As a result, it is a less-satisfying exploration of regional culture than those advanced, for instance, by John Shelton Reed and Norval Glenn.

On balance, however, these criticisms do not negate the considerable value of this book. In general, these essays are well written and quite informative; and Lea's topical choices and organizational scheme are firmly rooted in traditional concerns of southern politics research. While not all readers will agree with some of the analyses and conclusions, all should find much of the data and related discussions worthwhile. Lea and the authors of the individual chapters do a particularly good job of referencing other parts of the book and identifying common threads running through their respective analyses (such as, for example, the subcultural foundation of southern politics and recent changes therein), a sizable accomplishment, given the difficulties involved in bringing unity to an edited work. In summary, the volume, especially when taken in conjunction with other recent works in the field, is a worthy and useful addition to the literature on southern politics.

ROBERT P. STEED

The Citadel

Corwin on the Constitution. Vol. 3, *On Liberty against Government.* Edited by Richard Loss. Ithaca: Cornell University Press, 1988. 272p. $31.50.

The third and final volume of *Corwin on the Constitution* contains 12 essays, making a grand total of 48. One of them, a gem called "The New Deal in Light of American Political and Constitutional Ideals," appears in print for the first time here. Many of the others have long been regarded as classics and have been reprinted before. Constitutional lawyers, legal historians, and political scientists owe Richard Loss a substantial debt for pulling these scattered materials together and supplying both a table of cases and an index. It is unfortunate that he decided not to include Corwin's pathmarking empirical study, "The Extension of Judicial Review in New York, 1783–1905" (*Michigan Law Review* 15 [1917]: 281). It is also unfortunate that the table of cases and the index compile references only to Corwin's *text.* People, ideas, and cases discussed in his often eye-opening footnotes cannot be located with dispatch.

"If judges make law," Corwin once remarked, "so do commentators." Like James Kent and Thomas Cooley before him, Corwin sought to direct "the preponderance of legal opinion" (p. 146) with painstakingly crafted arguments that not only drew on history and philosophy but also addressed "the needs of modern complex society" (p. 53) as he understood them. All three ingredients may be discerned in varying proportions within each of the essays in this volume. Half of them focus on the rise, fluctuation, and fall of the vested rights and dual federalism doctrines, the principal "outposts" of a conservative constitutionalism that Corwin pilloried unmercifully until it perished in the upheaval of 1937. Three essays consider aspects of First Amendment interpretation, where the targets of his criticism include such "modern liberals" (p. 103) as Zechariah Chafee and Justices Holmes, Brandeis, Rutledge, and Douglas. "In general terms," Corwin wrote in 1950, "our system has lost its resiliency and what was once vaunted as a Constitution of rights, both state and private, has been replaced by a Constitution of powers" (p. 241). The air of wistfulness in this statement is unmistakable. Yet Corwin did as much as anyone to precipitate the transformation and to defend it as necessary, just, and consonant with U.S. constitutional values.

Loss's introduction is disappointing. He summarizes the gist of each essay serviceably enough; but he does not indicate how the pieces reprinted here fit in the larger corpus of Corwin's work, explaining, for example, why "National Power and State Interposition, 1787-1861," ought to be read with chapter 2 ("We the People") of *The American Doctrine of Judicial Review* (1914). Nor does he draw the reader's attention to reappraisals of Corwin's pioneering historical interpretations, ranging from Leonard Levy's critique of "The Basic Doctrine of American Constitutional Law" in *The Law of the Commonwealth and Chief Justice Shaw* (1957) to Gerald Gunther's attack in *John Marshall's Defense of* McCulloch *v.* Maryland (1969) on Corwin's penchant, evinced in "The Passing of Dual Federalism" and elsewhere, for invoking John Marshall's qualified constitutional nationalism in defense of unqualified New Deal centralization. Historical analysis, one of Corwin's strong suits, is not Loss's forte. Corwin's thought engages him, but the concerns that not only shaped Corwin's work but also influenced his research and rhetorical strategies do not. As a result, Loss is more inclined to make Corwin a participant in the constitutional conversations of our own era than to locate his work in historical time. Loss's main point, made initially in the second volume and reiterated here, is that the collected essays "show Corwin traveling full circle from his early rationalistic account of judicial review and complimentary attitude toward the founders' intention through his criticism of the founders' intention to end up criticizing the moral basis of the New Deal according to Chief Justice John Marshall's political thought" (p. 23). In my judgment, the collected essays show no such thing. Conscripting Corwin for service in the current pastimes of New Deal-bashing and "founders' intention"-proselytizing, moreover, is to engage in the very sort of sophism that Corwin delighted in unmasking when it emanated from John C. Calhoun, Stephen J. Field, George Wickersham, or Hugo Black.

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The Origins of American Constitutionalism. By Donald S. Lutz. Baton Rouge: Louisiana State University Press, 1988. 264p. $25.00 cloth, $12.95 paper.

Constitution Making: Conflict and Consensus in the Federal Convention of 1787. By Calvin C. Jillson. New York: Agathon, 1988. 256p. $30.00 cloth, $15.00 paper.

One of the more delightful spinoffs of the bicentennial of the Constitution is the increasing interest in, and visibility of, constitutional scholarship. Not since the days of Corwin, Wilson, and Haines (Can you name the last constitutional scholar who served as president of the APSA? Were you alive then?) has the field attracted such interest. Two welcome additions to this growing literature are Donald S. Lutz's *The Origins of American Constitutionalism* and Calvin C. Jillson's *Constitution Making: Conflict and Consensus in the Federal Convention of 1787.*

Lutz reads the federal Constitution of 1787 retrospectively, uncovering its foundations in the earliest U.S. expressions of community (e.g., the Mayflower Compact, the Pilgrim Code of Law, and the Fundamental Orders of Connecticut). These early protoconstitutions formulated ideas and principles that later informed the state constitutions and the Articles of Confederation—the most proximate influences on the federal Constitution. His claims on behalf of the state constitutions rest on two foundations. The first, that specific provisions of the Constitution have meaning only with reference to existing state constitutions (e.g., suffrage qualifications, procedures for selecting the Senate and president), is well taken and emphasizes once again the centrality of the states in the U.S. constitutional structure. His second claim, that the states provided useful models and experiences for the design of the federal Constitution, is less convincing. His own limited examination of the structures and procedures of those constitutions emphasizes as many differences as similarities. In short, Lutz's claim for the indispensability of the state constitutions for understanding the federal Constitution is a bit too convenient. Where he finds direct linkages between the federal and state constitutions, he asserts that it is clear evidence of the formative role of state constitutions. But where the federal Constitution departs dramatically from the state experience on some of the most important questions (e.g., in the adoption of a strong executive and a judiciary independent of direct legislative control), Lutz once again asserts that the state constitutions were important reference points—this time by revealing their weaknesses to the delegates. Much of this argument resembles "Heads, I win—tails, you lose." Ironically, Lutz makes a far more convincing case for constitutional paternity on behalf of the Articles of Confederation, noting that as much as "two-thirds of what was in the Articles showed up in the 1787 document" (p. 133).

What readers will find most compelling in Lutz's work, however, is not his examination of the immediate preconvention ideological map but rather his meticulous and enlightening perspective on the earliest U.S. constitutive documents (esp. chaps. 2-6). One might quibble about his choice of definitive authorities (for example, his idiosyncratic reliance on Theophilus Parsons's *The Essex Result),* but his interpretive style is always careful and forthrightly textual. It allows Lutz to claim effectively that U.S. constitutionalism is based on a text that is larger than the federal Constitution. Citizens created themselves as a people first through the Declaration of Independence and the Preamble to the Constitution. Taken together the two comprise a social compact. The establishment of a specific form of government was left to the rest of the federal Constitution. This transcendent view of U.S. constitutionalism is not new, but Lutz presents the case more convincingly than ever.

Lutz comes very near to accepting the exceptionalist vision of the U.S. constitutional tradition. He recognizes numerous European antecedents for the U.S. tradition, most notably the covenant tradition within dissident Protestantism, English common law and political understandings, the Enlightenment, and Whig political theory. But in the end Lutz claims that U.S. constitutional traditions are "ultimately planted firmly in American soil" (p. 166). While Montesquieu, Locke, Blackstone, and others were often cited by state and national constitution writers, Lutz warns us not to see them as the sources of U.S. constitutionalism but rather as convenient rhetorical authorities to justify already well developed "homegrown" political ideas.

Where Lutz gives surprisingly short shrift to the work of the 1787 Convention, it is the sole focus of Jillson's book. His project is to determine, by a sophisticated quantitative analysis of the roll call votes at the Convention, whether the delegates acted out of principle (a position supported by folks like Diamond, Wood, and probably Lutz) or out of interest (Beard, Roche, and Jensen). His analysis tracks shifts in the state coalitions and notes that on "higher" issues of constitutional design and structure the Convention divided into two ideological camps, one nationalist in vision, the other more suspicious of centralized power and protective of states' rights. Whenever the Convention dealt with specific allocations of power, however, the ideological coalitions broke down. On these "lower"-level issues state delegations pursued their own local advantages. These deadlocks could only be broken after the "lower"-level issues were submitted to carefully chosen committees, which, divorced from the passions of the main arena, were able to reach a compromise.

Each compromise created a realignment in which a new dominant coalition emerged. For example, Jillson finds that nationalists from the Middle Atlantic states were very successful in the early days of the Convention. But as issues of apportionment arose (issues that affected who would control the powerful new government) the dominant coalition broke down. Maryland and Delaware (potential losers in a proportional Congress) joined other small states in opposition to the large and soon-to-be-large states. This deadlock was broken by the Connecticut Compromise and a new dominant coalition emerged that had its way for several weeks until the next "distributive" problem arose. What is new here is not, of course, that there were compromises at the Convention, but rather that neither abstract principle nor concrete interests alone shaped the final document. Jillson believes that the delegates moved back and forth between principle and interest with ease. Ideology shaped the structural and purposive elements of the constitutional design, but local interests intervened when specific questions of power distribution arose (p. 17).

Even though both books are about the Constitution, direct comparisons are difficult. Lutz's approach is textually and historically grounded; Jillson relies on empirical methods for his conclusions. Lutz sees the Constitution as linked to its historical antecedents; Jillson examines it as a unique political phenomenon. Both claim they are indebted to Daniel Elazar's seminal observations about U.S. political subcultures. But, curiously, one searches in vain for any result in either work that is predicated upon Elazar's work. Lutz's own constitutional theory—a sort of superintentionalism in which not only the Supreme Court but all citizens should be bound by the values of the framers—is more transparent in his work, while Jillson's is more guarded. This is not intended as criticism. Indeed, by being more open about his intentions Lutz has made his book the more accessible and engaging of the two. Finally, neither book is as organizationally cohesive as one would like. Substantial portions of each book have appeared in professional journals previously. This is certainly no crime. But there are times when this patchwork structure creates transitions that are abrupt and passages that are repeated too often, as if the authors had forgotten what they had said in the last chapter.

None of these criticisms should deter serious constitutional scholars. Anyone who wishes to speak intelligently about what the framers intended in 1787 will have to confront Lutz's historical vision of U.S. constitutionalism and Jillson's roll call analysis of the Convention.

GLENN A. PHELPS

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No Longer Disabled: The Federal Courts and the Politics of Social Security Disability. By Susan Gluck Mezey. New York: Greenwood, 1988. 208p. $37.95.

Between 1980 and 1985, Social Security Disability Insurance was all over the newspapers. The Reagan administration had stepped up earlier efforts to cut back on the disability rolls by tightening eligibility standards and by reviewing the eligibility of those already on the rolls. About half a million beneficiaries were terminated. As the new administration's campaign picked up momentum, so did the lawsuits by rejected beneficiaries. Federal courts overwhelmingly supported the beneficiaries, attacking the Social Security Administration's (SSA) rules and procedures at the same time. What had been a battle between individual claimants and a giant welfare agency turned into a war between the executive branch and the courts. The Social Security Administration simply rejected the court's authority and declared a policy of "nonacquiescence," by which it refused to be bound by the court's opinion except in the individual case at hand. It forced new rejected beneficiaries to relitigate the same issues of law.

Susan Gluck Mezey analyzes this important slice of U.S. policy history from two vantage points. She is a lawyer who worked (somewhat reluctantly) for the Social Security Litigation Unit of the Department of Health and Human Services during the period of this upheaval. She is also a political scientist and wrote this book after she had returned to the professoriat at Loyola University in Chicago. Although there has been a fair amount of legal writing on the disability controversy, Mezey fills gaps in our knowledge about the roles of Congress and the SSA in the whole affair.

Students of bureaucratic and judicial politics will recognize many traditional themes in Mezey's interpretation of this case study. She demonstrates, for example, how a vague congressional mandate to review beneficiaries in the name of "administrative efficiency" was passed in 1980 with little deliberation—much less analysis—and left enormous discretion to the agency. When a new administration came in, bent on cutting back social programs, the same law became a tool for enforcing program austerity. She shows the importance of litigation as a mobilizing strategy for a mass of rather down-and-out, dependent clients who otherwise had no coherence as a group or any vehicle for organizing to preserve their benefits. And she shows the complex way that courts shape administrative policy. On the one hand, courts have substantial nominal authority to review administrative behavior; but on the other, they have no means of enforcing compliance. In the end, the courts' major influence was indirect. Rejected beneficiaries turned to their representatives for help. (Mezey quotes one staffer as saying that 90% of constituent work during this period was related to disability.) Congress ultimately took the courts' standard and wrote it into statutory law.

Buried in the details of what might seem an arcane bureaucratic and legalistic program are some fascinating glimpses of what happens when the welfare state meets the courts. One might wish Mezey had explored a bit more the significance of the disability controversy for these structural tensions of the welfare state. An early controversy in the program had to do with whether it would serve people who were disabled and also unemployed or only those who were unemployed *because* they were disabled. This was more than a question of grammar, it was a matter of how broadly the grounds for economic redistribution would be defined and how strictly unemployment would be considered as something within individual control, as opposed to structurally caused.

Another controversy concerns whether eligibility for government welfare programs is determined by individual assessments of each applicant or whether the state can use broad criteria, such as clinical diagnosis, age, education, and past work history to presume whether a person is able to work. Ironically, while the Social Security Administration has relied more and more heavily on categorical presumptions for the disbursement of disability-based income, civil rights law has pushed more and more in the direction of individualized determinations of ability in the employment of the disabled.

Still another underlying question has to do with the permanence of entitlements. By its enabling legislation and its staunch bureaucratic fiction, the program admits only the "totally and permanently" disabled. By amendment and practical reality, many beneficiaries are partially or temporarily disabled. So it is only fitting that the SSA reexamine beneficiaries. One issue raised in the litigation of the early 1980s is whether beneficiaries, once found eligible, can count on staying eligible unless the SSA can document some medical improvement in their condition or whether the entitlement is contingent, up for grabs every time the SSA wants to review it subject to the current standards, personnel, and political climate of the program.

For readers who are not already aficionados of disability insurance, this book may be hard to follow. As narrative it is quite disjointed, and some parts will be too technical for nonlawyers. One wishes publishers would provide better editorial guidance to authors whose closeness to their subject renders them wonderfully knowledgeable on substance but less able to judge a broader audience. All in all, this is a worthwhile book, and an important resource for those interested in courts and social policy.

DEBORAH A. STONE

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The Strategic Presidency: Hitting the Ground Running. By James P. Pfiffner. Chicago: Dorsey Press, 1988. 192p. $22.00 paper.

At the center of concern here is the transfer of presidential power in the United States, or, as Pfiffner puts it, "the abrupt changing of the guard prescribed in the Constitution" (p. 1) when a new president takes office. These transitions, he argues, are more difficult to make in the United States than in other modern democracies. In this country there is no shadow cabinet waiting in the wings to take over the task of governing if such an opportunity arises. Nor is there sufficient trust in the competence and political neutrality of the career bureaucracy to allow it to hold the reins of power while a new administration settles into office.

According to Pfiffner it is most difficult to make such a transfer of presidential power when an election results in one political party taking over control of the White House from the other. And it is on this kind of presidential transition that much of this book focuses. Unfortunately, the experience of the Bush administration was not available to Pfiffner when he wrote this book. Its performance after the Republicans won the 1988 election suggests that a presidential transition can be highly troublesome even when a political party holds on to the White House.

The chief task facing a new president, Pfiffner argues, is to devise strategies that will enable him to convert the authority he receives on Inauguration Day into the power he expects to exercise once he is ensconced in the White House. He defines the term *strategic presidency* to mean "a self-conscious approach to planning the assumption of governmental control after an election" (p. 4). He identifies Jimmy Carter and Ronald Reagan as the two modern presidents who were most assiduous in carrying out this kind of strategic planning.

The object of such advance planning is to enable a new president to "hit the ground running." There is some urgency attached to a president's doing such planning well before he takes office, Pfiffner contends, since the early days in office—the so-called honeymoon period—provide a unique window of opportunity for presidential accomplishments. If true, this proposition confronts the United States with the unsettling fact that its government is most capable of achieving its goals when it is least knowledgeable about how to do so effectively, a cycle that Paul Light has traced in his book, *President's Agenda* (1982). Moreover, the description that Pfiffner provides of the huge and somewhat unruly "adhocracy" that Reagan assembled to guide his transformation from candidate to president suggests that strategic planning has perils of its own.

The main body of the book spells out the things a new president should or shouldn't do in going about the task of organizing a government. It thus follows in the path that Neustadt long ago blazed in his classic book, *Presidential Power* (1960). We have learned something since then about the techniques available to modern presidents for succeeding, or at least looking good, in the White House; so Pfiffner's book is a very interesting supplement to the Neustadt volume (though no book on the presidency is likely to have quite the eye-opening impact that Neustadt's had).

Perhaps the most valuable feature of this book is that it helps us get an early fix on the Reagan presidency. As discussed by Pfiffner, one of the most distinctive features of the Reagan White House was the close control it exercised over appointments at all levels of the executive branch and the extent to which it was thus able to promote a degree of coherence in policy outlook among executive decision makers that was quite unusual in the modern presidency.

The most striking institutional innovation of the Reagan administration (saving perhaps the attempt to privatize foreign policy making in the Iran-Contra affair) was the creation of cabinet councils, small groups of cabinet members with diverse interests in common problems. These councils were created to bring about coordinated solutions to policy issues where cabinet secretaries might otherwise be working at cross-purposes. Since they were staffed by the White House, they also helped to bridge the gap that ordinarily separates the two main cadres of presidential appointees: those who serve in the White House and those who run executive agencies. This gap has been a persistent source of friction in Washington and an obstacle to the success of national policy making.

These councils seemed to work reasonably well in Reagan's first term, when White House staff members like James Baker and Edwin Meese provided them with strong leadership. They fell on hard times, however, in the second term, when Baker and Meese themselves became cabinet members and promptly lost all interest in allowing the White House officials who served on these councils to intervene in departmental matters. Whatever other changes the Reagan Revolution may have wrought, it was not apparently able to repeal the ancient bureaucratic maxim, "Where you stand depends on where you sit."

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Voters, Elections, and Parties: The Practice of Democratic Theory. By Gerald M. Pomper. New Brunswick, NJ: Transaction, 1988. 320p. $29.95.

The 20 articles and excerpts from books that comprise this collection of Pomper's work cover a little over two decades. To these he has added an essay, "The Practice of Democratic Theory," and introductions to the three parts into which the book is divided. The strongest of these parts is "Voting." It benefits from the theory chapter more than do "Elections" and "Parties," the other two parts; and it opens and closes with particularly strong articles. These are Pomper's classic revisionist study, "From Confusion to Clarity: Issues and American Voters" (*American Political Science Review* 66 [1972]) and his analysis, "The Impact of *The American Voter* on Political Science" (*Political Science Quarterly* 93 [1978–79]). In addition, Pomper's hallmark of innovative research methods applied to testing relationships of consequence to democratic theory is consistently evident here.

The theory chapter sets a framework for research on voting, elections, and parties, particularly research that may have practical applications. But it does not specifically place the articles that follow within the coordinates of that framework and invite the reader to pursue linkages between particular articles. For me "From Confusion to Clarity" (chap. 2) may be treated as the key to the book's unity. The analysis of the relationship of issue preferences to party identification, to awareness of party differences on issues, and to voters' consensus on party issue positions in the presidential elections of 1956-68 in this article has direct ties to articles in parts 2 and 3. The analysis reveals "increased voter consciousness of policy questions in the 1964 election and the later electoral persistence of group divisions based on [those] policy questions" (p. 42), thus supporting the conclusion in Pomper's 1967 article on election classification that 1964 marked a critical election and a new epoch in U.S. politics (p. 194). It was also in 1967 that his article on party platforms appeared (chap. 9), with its finding that these documents do have meaning and indeed may constitute agendas for future governments. Later demonstration in the 1972 voting article that voters can perceive the ideological character of the two parties and differences between them made it meaningful to view parties as "'groups of like-minded men' offering particular stances toward public issues" (p. 44). Thus, in 1972, Pomper established a much more satisfactory theoretical base for the analysis of party platforms than the Downsian reasoning that he originally invoked.

Parties that represent issue positions may fulfill the ideological condition for responsibility. But also needed are coherence, effectiveness, and discipline (p. 44), which suggests the need to assess the capacity of the major parties to demonstrate these traits. This is the pervading concern of part 3. The bridge between "Voting" and "Parties" is particularly important, for it transports author and reader between two dissimilar approaches that in the context of the book are quite complementary. In the party articles Pomper uses descriptive statistics to support inferences about party and in turn these inferences provide foundation for advocacy of "a public policy toward party itself" and for launching a movement "to improve the standing and effectiveness of the parties [through] a deliberate program of party renewal" (p. 250). This movement has met with litigation success, as the constitutional right of parties to admit independents to their primaries and to endorse primary candidates has been sustained against contrary state law in Connecticut and California.

Pomper describes the book as "a hybrid of an intellectual resume and a midlife autobiography" in which the author not only collects but reflects on his work (p. xiii). Graduate students and colleagues would find the book's meaning to them enhanced by more extended autobiographical reflection patterned on the "Retrospect" written in 1986 and included as chapter 3. Scruples about tampering with the structure of work that has already been judged and had its influence should not preclude insertion of personal commentary on the genesis of some of the articles and problems and satisfactions in bringing them to completion. But as it stands the book stimulates profitable reflection on an era of U.S. political science.

CORNELIUS P. COTTER

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The Politics of Glamour: Ideology and Democracy in the Screen Actors Guild. By David F. Prindle. Madison: University of Wisconsin Press, 1988. 240p. $24.50.

Prindle has produced a well written account of the emergence of the Screen Actors Guild (SAG), the Hollywood-based union that represents film actors. In Prindle's view, the SAG is an excellent labor organization in which to study the century-old conflict in the U.S. labor movement between "progressive" and "conservative" ideologies about the nature and purpose of worker organizations. What makes the SAG an important labor organization to study is that ideological conflict exists within a competitive political context unmarred by self-interest and self-seeking on the part of participants in the struggles, for guild officers receive no pay and no career advantages from their positions.

Prindle illustrates how the ideological divide in the SGA has structured how the union has tried to cope with such challenges as the changing political economy of the entertainment industry, including the breakup of the old studio system; the introduction of new technologies of production; the McCarthy-inspired blacklisting of leftists in the 1950s; and the emergence of competition from network and cable television and VCRs. Prindle also documents how the ideological split has shaped how the union has (or has not) responded to larger domestic and international social issues such as sexism, racism, economic inequality, and revolutionary movements in Central America and Southeast Asia.

Scholars interested in the internal political processes of unions will find his discussion of the emergence, consolidation, and eventual electoral victory of a progressive coalition inside the SAG fascinating and informative. In his discussion of the rise and victory of the progressives in the 1970s, who managed to displace the conservative faction that dominated the SAG from its founding, Prindle makes a major contribution to social science's understanding of structural factors that are conducive to the development of competitive union politics. The author concludes his study on a pessimistic note. He argues that unless the ideological cleavages between progressives and conservatives can be breached so as to permit strategic cooperation in the face of an ever-changing industry, the existence and effectiveness of the union will be seriously threatened.

Had the analysis stopped here, Prindle would have produced a work that might have stood as a model of how social science ought to approach the case study of a union. While one might disagree with some of Prindle's emphases (for example, his tendency to blame national union leaders for the labor movement's problems in the 1970s and 1980s rather than also examining the major onslaught of capital, with the support of the state, against the working classes in general and unions in particular), there is little for which Prindle can be criticized in the first seven chapters of his book.

In chapter 8, Prindle attempts to explain how any given actor comes to identify with either the progressive or conservative camp in the SAG. He relies on a neo-Weberian explanation to account for such an identification, arguing that the actor whose religious background and career experiences have tended to foster individualism will tend to identify with conservative ideology, while an actor whose religious background and career experiences require cooperative activity (as stage acting, for example, does) will identify with progressives. A series of bivariate tables was analyzed to test these hypotheses. The data came from 56 SAG members who had been politically active in their union.

In examining these hypotheses Prindle has made a serious statistical error. He uses Gamma to indicate the strength of association between a number of variables and whether or not one is a progressive or conservative. Unfortunately, Gamma is the wrong statistic to use. All of the variables (e.g., where career began, acting school attendance, type of work engaged in most often, type of work preferred, general political ideology, and religion) are nominal variables, as is whether one is a progressive or conservative. Gamma is a symmetric measure of association that requires all variables to be measured at least on the ordinal level. Prindle's data do not satisfy this assumption. A correct measure of association would have been the uncertainty coefficient or log odds ratios. In calculating appropriate odds ratios, one finds that the direction of effects is the same as Prindle found it to be. However, there are differences in the relative importance of some of the variables. For example, using Gamma, Prindle found that where career began, acting school attendance, and type of work preferred all were more important than parent's religion. The log odds ratios do not indicate that this is the case. The difference between Protestants and Jews with respect to ideology is as strong as the film-versus-stage contrast in the type-of-work-preferred table (p. 201) and is stronger than the contrast for acting school attendance. Also, calculation of log odds ratios for the tables for the effects of where career began and for general political ideology on whether one is a progressive or conservative (p. 202) shows that the Los Angeles-versus-*other* contrast has about the same consequence as does a liberal-to-middle contrast in general political ideology in differentiating between progressives and conservatives.

Such findings call into question Prindle's data that he reports in Table 8.4 (p. 199) and all of the conclusions about the relative importance of the various predictors he derives from it. Prindle has not supplied the reader with the cross-tabulated data necessary for computing appropriate coefficients in order to see if the inferences he derives are warranted.

Because of the statistical errors in chapter 8, the reader has no way of knowing whether Prindle is correct in his concluding assertions about the effects of various biographical and career contingencies on political ideology with respect to the guild or not. Therefore, the ultimate value of this work is seriously compromised; and the reader can feel nothing short of sadness at the work's unfulfilled promise as a significant contribution to our understanding of the factors that lead to ideological divisions within trade unions.

THOMAS J. KEIL

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The Acid Rain Controversy. By James L. Regens and Robert W. Rycroft. Pittsburgh: University of Pittsburgh Press, 1988. 248p. $29.95 cloth, $12.95 paper.

A comprehensive treatment of the subject of acid rain has been long overdue. While discussion of acid rain policy has appeared in books on energy or the environment, for example, Richard J. Tobin's "Revising the Clean Air Act" (in *Environmental Policy in the 1980's*, ed. Norman J. Vig and Michael J. Kraft [1984]), Walter A. Rosenbaum's *Environmental Policy and Politics* (1985), and B. A. Ackerman and W. T. Hassler's *Clean Coal/ Dirty Air* (1981), this welcome book focuses on the interrelationships of politics, technology, science, economics, and policy making as they influence the definition as well as alternative resolutions.

Acid rain is a classic example of the impact of end-use energy on the environment. The authors address the complexities of the acid rain problem as it affects multiple media, occurs over time, transcends political boundaries, and is associated with large litigation costs and uncertain benefits. The limits of science and the utilization of scientific information for political purposes are also examined. The political dimensions of acid rain involve the reform of existing air pollution legislation, government support of research and development, implementation of specific control measures and the competing interests concerned with coalition building.

The authors critically review existing and potential control technologies as well as strategies for allocating costs ranging from incorporation of cost into the rate base to more equitable approaches, such as a generation fee and sulfur content in fuel tax.

Special attention is given to less widely known initiatives taken by states to reduce sulfur emissions (New York and New Hampshire) that could serve as models for national policy making. The authors stress the global aspects of acid rain, emphasizing that the United States will eventually have to participate in efforts being undertaken in Canada and Europe to voluntarily reduce harmful emissions. Recognizing institutional constraints and disagreement among the Reagan administration, the Environmental Protection Agency, and Congress about the causal relationships involved in producing acid rain and the degree of severity of the problem, there is opportunity for optimism. The authors suggest that intensified environmental interest group activity, media attention, and the U.S. tendency to take action when a problem receives public attention will force authoritative action. Greater public awareness of the importance of the acid rain problem can create an environment that compels political institutions to take innovative approaches in an attempt to satisfy public concern as well as deal with a problem while it is still manageable.

The Acid Rain Controversy is well written and clearly organized. The discussion of the Clean Air Act and alternative control technologies are particularly informative and useful. The book has special value, because of its interdisciplinary and international perspective, to students of public policy and those interested more specifically in the relationship of energy and the environment. It also provides an excellent context for examining acid rain from a regulatory policy dimension. The extensive references and appendices are especially worthwhile additions that authors oftentimes neglect.

REGINA AXELROD

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"To the Best of My Ability": The Presidency and the Constitution. By Donald L. Robinson. New York: W. W. Norton, 1988. $8.95 paper.

Constitutional reform is a staple of the literature on the presidency. Reformers in the nineteenth century, such as Henry Lockwood or Augustus Woodward, proposed to eliminate the office or turn it into a ceremonial position while Congress did the real work of governance. Soon after (professor) Woodrow Wilson called for cabinet government and Albert Hart forecast the emergence of "Speaker as premier," Presidents Roosevelt, Taft, and Wilson were changing the contours of the presidency and enlarging its diplomatic and military prerogatives. The prescriptive literature in modern times decries the emergence of an "imperial" presidency and suggests reforms (a question period for cabinet secretaries, restrictive legislation on war and diplomatic powers and covert operations, new patterns of interbranch policy codetermination), or a revamping of the party system to provide for responsible party government. While the political scientists are prescribing, presidents from Truman through Reagan have developed the national security state and engaged in constitutional excesses involving undeclared wars, covert operations (including attempted assassinations), bypassing of mandated congressional overseeing, use of nonappropriated funds, and domestic surveillance and disruption of the political process.

Donald L. Robinson, director of research for the Committee on the Constitutional System, has written a book that does two things: in 11 chapters he skillfully describes how presidents are neither effectively meeting their constitutional responsibilities nor remaining accountable to the people in their exercise of power, and in one final chapter he offers a number of ideas on how to make the presidency—indeed the entire apparatus of government—conform to the "fundamental constitutional values which we share with the founding generation" (p. 263). These values are twofold: "Power must be checked and balanced, and authority must be kept accountable to popular will" (p. 263). The 11 chapters range widely from a splendid disquisition on the experiences of colonial and state politicians prior to the Convention of 1787 to presidential fumbling with economic management in the 1980s. Robinson is as tenacious as a pit bull when it comes to sticking close to fundamental issues of political theory. He makes original points about Harrington, John Adams, and Theophilus Parsons; and he also provides wonderful insights on almost every page. (Montesquieu used the term *separation* only once in his treatise; the disenfranchisement of slaves pushed the southern states toward an electoral college rather than popular elections; the Bureau of the Budget and Council of Economic Advisors were originally designed as constraints on the presidency.)

The first 11 chapters clear away the underbrush and prepare the reader for the final chapter, in which Robinson makes four proposals for constitutional reform that would "remove features which serve primarily as a deterrent to coherent political action and accountability" and that would "provide a way to resolve deadlocks between the branches" (pp. 269–70). His proposals include the following: (1) allow members of Congress to serve in administrative offices; (2) provide four-year terms for members of the House and eight-year terms for Senators, with election of all House members and half the Senators at each presidential election; (3) allow the president to call new elections during his term (provided at least one-third of the House and Senate concur) and allow Congress the same power by majority vote of each chamber; and (4) establish a "federal council" consisting of "100 notable persons" chosen for life by the president, who in turn would choose one of their number to serve as "chief of state" for a term of two years, an officer who would superintend the power of dissolution. These proposals stop well short of the "party responsibility" school or of those (such as Lloyd Cutler) who argue for a modified form of cabinet government, and they are different from the consensus proposals made by the Committee on the Constitutional System.

I remain unpersuaded about the merits of these proposals. Robinson argues that the administrative process has become too "legislative" because of the existence of iron triangles that first pass laws with too much discretion and then control their administration. It is hard to see how allowing legislators into these administrative agencies will solve that problem: one might argue that if a version of Lowi's "interest group liberalism" is the problem, Robinson's reform is somewhat akin not only to letting the fox into the chicken coop but making him into the chicken farmer. The four-year and eight-year terms are designed to maximize presidential coattails; but while the system of party competition remains so asymmetrical (due in part to gerrymandering House districts, in part to constituency service and PAC giving patterns), it isn't clear that such a reform would give the president party majorities. And even if the president had them, would it solve the problem of party government? Weak legislative party discipline and Madisonian "ambitions counteracting ambitions" have long frustrated presidents, even those with large majorities (e.g., Lyndon Johnson) and for sound constitutional reasons, all of which Robinson has skillfully described in the preceding chapters. The proposal for dissolution, as Robinson himself notes, flies in the face of our accustomed four-year intervals, which are so ingrained as almost to define our sense of political time.

Finally, the notion that the "wise persons" should be named to a council after their service in government reminds one of the original hopes that the electoral college would consist of the "best and brightest." If the president appoints, he is likely to fill the council with defeated members of the House and Senate and former White House aides, cabinet functionaries unknown beyond the Beltway, and the sort of people who wind up on blue-ribbon panels. A group of retreads isn't likely to create respect for the council or its "chief of state." It isn't likely the council would be nonpartisan either when the presidency is dominated by one party for two decades, and this would weaken its legitimacy as an overseer of the dissolution process. If the role of the council is merely to superintend the dissolution process, we can always (by constitutional amendment) give that duty to the Supreme Court instead, which at present has the franchise on the "wise men" function in our system, or, better yet, provide a statutory commission with bipartisan membership.

At one point Robinson says that "the longer you examine an issue, the more complicated it becomes" (p. 281). That holds true for his four reform proposals (and even more for his tantalizing footnote suggesting a unicameral legislature), which at the very least are valuable insofar as they force skeptics to frame a response. Reform proposals aside, Robinson has produced a fine analysis of the evolution of the presidency that matches the enduring questions of political theory against the sorry performance of the institution in modern times.

RICHARD M. PIOUS

Columbia University

A Century of Judging: A Political History of the Washington Supreme Court. By Charles H. Sheldon. Seattle: University of Washington Press, 1988. 379p. $35.00.

There are few contemporary parallels to this attempt to provide a comprehensive description and analysis of the composition and work of a single state's supreme court from the year of the state's admission to the Union. It is an ambitious task.

Sheldon devotes approximately 60% of his book to a detailed description of the process that led each of the 77 members of the court to the bench. Many were originally appointed and then ran for election to a full six-year term, and the author describes both events. The remainder of the book then examines the operation of "representative" courts for five time periods. For each of them he examines what he calls "goal achievement" and "court viability," which are evaluations of the quality of the courts' work as measured by subsequent citations, of cohesiveness on the court, and of the effectiveness or the dispatch of their business.

The book is full of details that would otherwise be unavailable to the student of this court. Indeed, I know of no comparable work of similar sweep. One can learn here the circumstances under which every judge was elevated to the bench and many details of how his court did its work. It is a gold mine for subsequent researchers.

Yet Sheldon's book has serious limitations that will limit its audience. The first is that despite its sweep, it does not inform us about either the Washington court's contribution to the policy outputs of the state's governmental machinery or its role in the development of jurisprudence in the United States. For instance, although Sheldon refers to the more significant role of state supreme courts in the 1970s and 1980s, he does not tell us whether the Washington court has used the Washington constitution to provide further protection of civil rights, nor does he tell us of its role in the contemporary development of tort law or other legal doctrines. The same is true for earlier periods of the court's work. The interplay between court and legislature in the development of public policy is simply not reflected in this book because the author has not examined landmark decisions of the Washington court or conflicts between the court and the legislature with the same thoroughness that marks his treatment of the selection process.

The second limitation is that Sheldon's theme, the tension between public accountability and judicial independence, is not an explanatory scheme. There are many illustrations of this conflict; but it does not summarize the contents of the book, nor does it provide an understanding of the court's work. Consequently, the reader is swiftly overwhelmed with detail: endless names not only of the judges selected but also those of their rivals, the particular circumstances of their elevation, and (in later pages) the rules and procedures of each court and summary accounts of the court's performance on the standards the author employs. It is simply too much for any but the most determined reader.

While Sheldon's book cannot be recommended to the casual observer of state politics or state judicial processes, it nevertheless stands as an important contribution to scholarship on state courts. It is not the last word and Sheldon did not intend it to be that. *A Century of Judging* is a significant building block in the arduous task of understanding how U.S. state courts contribute to policy and law.

HERBERT JACOB

Northwestern University

Automatic Government: The Politics of Indexation. By R. Kent Weaver. Washington: Brookings Institution, 1988. 276p. $31.95 cloth, $11.95 paper.

Automatic Government is concerned with explaining the increase in the use of indexing—automatic adjustments for inflation—in federal programs. Despite the loss of discretion for policy makers, by 1980 they had indexed more than 90 federal programs directly or indirectly to the Consumer Price Index (CPI), constituting about 50% of the federal budget. Consequently, a 1% increase in the CPI produced an additional three billion dollars of federal expenditures. In addition, once programs became indexed, they have remained so, even during the Reagan administration's budget-cutting efforts.

Why has such widespread indexing occurred, and what are its consequences? Weaver rejects single-variable explanations (such as the argument that indexing reduces decision-making overload) as overly simplistic and inadequate to account for the variety of programs indexed and their consequences. His explanation relies on a rational choice logic to construct a theory of indexing that includes members of specialist committees, budget committees, chamber majorities, party factions, the president, and interest groups. The theory posits a relationship between these participants, their dominant objectives and strategies, and the resultant outcomes. Each policy involves a somewhat different set of participants with differing combinations of objectives and strategies, such that the outcomes vary from one policy to another.

Participants have four dominant objectives: claiming credit, avoiding blame, trading votes, and implementing good policy. Which participants pursue which objectives depends on several factors, including the strength of the clientele group, the prospects for discretionary increases in the program's budget, the position of the president, and the potential blame for supporting or opposing indexation. Policy specialists, for example, face pressures to vote for indexing even though it will reduce their opportunity to claim credit for legislating popular increases in the future. The short-term calculus of these specialists involves the possibility that the clientele group that favors indexing will blame them if they vote against its adoption.

The book also offers a rich, detailed description of six policies. There is a separate chapter on congressional pay, social security, dairy price supports, food stamps, minimum wages, and the federal income tax. Each chapter describes the indexing mechanisms, the program characteristics, expenditures, and revenue implications. The chapters are highly valuable for understanding the variety of indexing mechanisms and the widely varying circumstances under which indexing has been adopted and maintained or reduced. These chapters also present a convincing set of analyses of the political calculations, objectives, and strategies of the participants in each policy area.

While the book uses a rational choice logic, it does not contain any formal analyses of relationships or of strategies. For those with an interest in game theory or formal analyses, however, the book offers a mine of hypotheses and ideas. The book also contains no sophisticated quantitative analysis, although growth trends in expenditure as compared to gross national product and similar descriptive statistics are employed. This lack of quantitative analysis limits somewhat the chapters on the consequences of indexing, which focus not on the economy or society but on the decision process, including its accountability, representativeness, and fiscal responsibility. However, Weaver does show differences between programs. For example, Aid to Families with Dependent Children (AFDC), which was not indexed, fared much worse in budget growth than food stamps, which was indexed.

There is no full recognition of the importance of the status quo in determining the hurdles policy makers must overcome in order to pass new legislation versus automatic increases. (The term *status quo* does not appear until p. 253.) Defending an existing policy is much easier than proposing changes in the status quo, which must go through numerous institutional hurdles. In addition, there is an important distinction to be made between policies as statutes and policies as implemented. Many forces outside of the federal government determine the effects of policies on citizens. If a congressional committee sees a policy's program level erode due to inflation, it must propose new legislation in order to reestablish the program levels of the original statute. Such new legislation is much harder to accomplish than defending an already-passed statute.

The book would benefit from a summary chart or matrix linking the six programs with the four objectives for the differing participants. Such a summary matrix might stimulate integrative thinking about these diverse policy choices. While Weaver is probably right in concluding that there is no *one* politics of indexation, the general conditions under which one form of politics occurs versus another would further our understanding of the whole without relying on a single-variable explanation.

Automatic Government is a very stimulating and readable intellectual argument about the politics of policy choices in the federal government. It is also a very careful and detailed piece of scholarship that investigates a wide range of cases. The book is a valuable work for students, teachers, and officials who are concerned with budgeting and public policy. Its contents will prove to be informative for economists interested in indexation and the economy, politicians interested in understanding their own and their institutions' behavior and its consequences for budgets and programs, and generally informed citizens who see the growth in this phenomenon of indexation but do not understand fully its causes or consequences.

JACK H. KNOTT

Michigan State University

Urban Reform and Its Consequences: A Study in Representation. By Susan Welch and Timothy Bledsoe. Chicago: University of Chicago Press, 1988. 154p. $27.50 cloth, $10.95 paper.

The municipal reform movement of the early twentieth century succeeded in bringing many structural features to city government in the United States. Upper-class, native, Protestant reformers sought to curtail the power of political bosses and their lower-class, immigrant constituents. Nonpartisanship and at-large districts were important structural weapons in the arsenal of the reformers. They hoped that nonpartisanship would tilt the urban political agenda toward economy and efficiency in public service and away from the redistributional demands of lower-class populations. At-large elections were designed to bring "better people" into municipal government and restrict the influence of residentially segregated immigrant groups. Citywide districts were expected to deemphasize neighborhood issues, minimize constituents' service work, and reduce political conflict. This would free council members to focus on citywide concerns, notably economic growth and business prosperity. Certainly the reform movement was successful in getting its structural devices adopted. Over 70% of U.S. cities have nonpartisan elections and over 66% have at-large districts.

Are these structural "reforms"—nonpartisanship and at-large districts—having their intended effects in city politics? Susan Welch and Timothy Bledsoe have undertaken a systematic inquiry into the consequences of urban reform. They summarize the historical origins of the reform movement, carefully describe its theoretical underpinnings, systematically derive behavioral propositions from its theory, and test these propositions against the responses of 975 council members across the United States to a mailed questionnaire.

The book compares the reports of council members elected on partisan and nonpartisan ballots and in at-large and district elections, regarding their recruitment, party identification, and socioeconomic background; their "representational focus" (the city as a whole or neighborhood and ethnic constituencies); their attitudes toward constituent services; their perceptions of conflict and factionalism; and their policy positions on recurring urban issues. Appropriate controls are employed to assess these structural differences' independent effects on council members' responses.

The book is so clearly written, theoretically rich, and well integrated into the existing scholarly literature in urban affairs that the reader hardly notices the thinness of the data on which the findings are based. The findings are based on a single questionnaire mailed in 1982 to sixteen hundred council members in 218 cities with populations between 50 thousand and one million. The well-designed questionnaire (reproduced in the appendix), with its concise and close-ended questions, was rewarded with a 61% response rate. Among the findings are that district council members report that they spend more time on constituent services than at-large council members and that they focus more attention on neighborhood or area problems, while at-large council members appear to focus more on citywide and business constituencies. No consistent differences were found on policy issues. The results are honestly reported: "Most of the differences among types of systems are fairly modest" (p. 119). Indeed, the only awkward scholarship occurs in the final chapter, where the authors engage in some unwieldy speculation as to why they found only modest differences.

The structural issues raised in the Progressive era have taken on a new urgency in city politics today. Newly mobilized minorities in urban politics have subjected the traditional "reform" theories of representation and good government to new criticism. It is the continuing responsibility of social scientists to inquire into the political consequences of structural reforms. Welch and Bledsoe concisely and accurately summarize the work of earlier scholarship on these questions, notably the adverse effects of reform on black representation in cities. And their well-constructed report of council members' perceptions of representational issues makes a valuable contribution to our understanding of the consequences of urban reforms.

THOMAS R. DYE

Florida State University

Where We Live: A Social History of American Housing. By Irving Welfeld. New York: Simon & Schuster, 1988. 288p. $18.95.

This is not a modest book. The inaccurate subtitle does not appear on the dust jacket, where it is replaced by *The American Home and the Social, Political, and Economic Landscape, from Slums to Suburbs.* Like the title, the book is a little fuzzy at the edges—mainly true, but not quite on the mark. Its author is a long-time analyst at the Department of Housing and Urban Development, a lawyer by training, with a reputation for being an intellectual curmudgeon at the department. The book is a knowledgeable, opinionated (in both the good and the bad sense) review of a wide variety of issues in contemporary U.S. housing policy, iconoclastic on some issues, accepting a great deal of the conventional wisdom on others, ending with some provocative, if not earth-shaking, prescriptions, the whole self-consciously written in what its author calls the "vernacular," a welcome change from heavy Academese even if the wit is sometimes on the forced side.

As an introduction to the economics of housing and the policy considerations currently occupying governmental attention, the book is excellent, perhaps the best around. Its 12 chapters review the construction process of housing and the pros and cons of manufactured homes, the mortgage system, the difficulties of rental housing, the reasons for suburban sprawl, some aspects of the relation between job changes and housing, housing and racial discrimination, the extent of the present and past housing shortages, the major federal housing programs, demand-side subsidy efforts, the secondary mortgage market, and housing and inflation. Some of his recommendations are sensible, some innovative, some old hat: financial incentives to prompt the elderly to move out of too-big units, making them available to those who need the space more (a good idea if hardly novel); varying rents in public housing by quality of unit (theoretically a sound idea if full of practical problems); and different formulas for distributing rental construction subsidies and housing vouchers (worth thinking about). Welfeld has been around a long time, he has been near the centers of the action, he has kept his eyes and ears open, and he has been willing to be unorthodox; so much can be learned from his book.

But at the same time the book is hardly a real history; omits major aspects of the landscape of housing; and relies on rather conventional economic theories about the filtering process, the rational behavior of housing consumers, the separation of political decisions from market processes, and indeed the smooth functioning of those processes. The sociological issues in housing are barely touched on, neighborhood change is barely mentioned as the growing issue that it is, the effects of continued poverty and racial discrimination on their victims is hardly examined, and the existence of an "underclass" is blithely assumed without reference to the bitter debate about its validity. Indeed, references to the opinions of others are not Welfeld's strong suit; nor is the extensive research on some of the matters he discusses often brought into play. The debates about homeownership for the poor, which have highlighted some of its negatives; the questions about how far the filtering processes (by which better homes are made available to lower-income households as their residents move up the quality scale) really works and whether quality does not fall as lower-income households move up; what the future of the city is and whether it is "obsolete," simply a "transitional stage between rural and suburban" (p. 86), as Welfeld claims, or not; how many homeless there are; whether demand-side subsidies inflate housing prices; how desirable rent-income ratios might be intelligently calculated—these are all issues about which much has already been thought, substantial empirical evidence exists, and others have written intelligently but about which very little is disclosed in Welfeld's book. And on a few points more scholarly completeness would have seemed almost obligatory. That the major source of information for a section headed "The Poor Landlord As Hero" (p. 154) is data commissioned and paid for by an organization of landlords, for instance, would seem to require at least a footnote acknowledgment.

The one place where Welfeld's lack of discussion is most to be regretted, however, is where he could, presumably, have contributed the most: the ins and outs of policy making within the federal establishment and the relationships among administrative decisions, congressional and presidential politics, and interest group pressures. Perhaps, while he is still at HUD, these are unreasonable expectations to have. Given the breadth of his knowledge and experience, his sharp tongue and willingness to challenge shibboleths, if Welfeld wrote more of his own immediate environment, his book might really be ground-breaking. As it is, while it is less than what either of its subtitles proclaims, it is still an excellent introduction to, and a provocative if limited policy discussion of, issues that will only increase in importance as the century draws to a close.

PETER MARCUSE

Columbia University

Stalemate: Political Economic Origins of Supply-Side Policy. By Howard A. Winant. New York: Praeger, 1988. 224p. $39.95.

President Kennedy's tax bill, proposed in 1962 and enacted two years later, sharply cut tax rates in an attempt to spur economic growth. President Reagan used this Revenue Act of 1964 as a model for his 1981 supply-side tax cuts, and Winant dissects it as economic policy making. Both Kennedy and Reagan did what they pledged, though neither succeeded quite as completely as might have been hoped.

In examining the political and economic origins of the Revenue Act within the Kennedy administration and its modification and adoption by Congress, Winant asks three questions: Why was comprehensive tax reform needed? How was a comprehensive tax reform developed? What explains congressional treatment of these comprehensive reforms? To address these questions, Winant attempts to synthesize *class*, *elitist*, and *pluralist* theoretical perspectives. In the two chapters of part 1, he presents the strengths and weaknesses of these theories of politics. Part 2's four chapters are devoted to the elite politics of the executive branch as it puts together the Revenue Act proposals. The next five chapters in part 3 describe the pluralist politics of Congress as it takes the Revenue Act apart. In the final chapter, Winant returns to a class analysis in "The Advanced Capitalist State and Economic Policy Formation."

Pluralist theory largely explains congressional activity. Congress, in particular the House Ways and Means Committee and the Senate Finance Committee, modified their president's proposals in response to interest group demands. Ways and Means' chairman Wilbur Mills crafted a bill not from some comprehensive view of economic rationality but with a sharp eye toward building a solid majority coalition to support it. In Winant's eyes, no theory purporting to explain economic policy making can safely ignore pluralism's insights. However, he finds pluralism lacking on a major point: When neither Congress, interest groups, nor the public were calling for broad tax reforms, how did they get on the agenda in the first place?

Winant turns to elite theory for an answer. The executive branch perceived slowing economic growth as a barrier to successful completion of its policy goals. Stimulating economic growth was a technical matter that could be solved by rationalizing national economic policy. Comprehensive tax reform was one way to do this. Charismatic Kennedy (foreshadowing Dukakis, not Reagan) based his reforms not on the "grand warfare of rival ideologies" but the "practical management of a modern economy" (p. 63). A narrow elite within his administration consequently proposed tax cuts from its analysis of the "new economics," not in an ad hoc way to satisfy particular interest groups. Still, elite theory is quiet regarding the source of the economic problems the comprehensive reforms are intended to remedy.

Class theory is not silent on this point. In the class perspective, capitalist economies are inherently prone to stagnation and crisis. The state attempts to act as the ideal collective capitalist to ensure the conditions necessary for capitalist reproduction. After all, the state's survival is tied to capitalism's. The state cannot permanently solve capitalism's problems, however. Its ability to act as ideal capitalist is frustrated—as Kennedy's administration was—by the ability of particular political actors to gain their narrower immediate objectives. Nor can the state provide any lasting solution because each economic policy "gives rise to new problems, new demands, and new instabilities in the equilibrium it seems to establish" (p. 33).

Stalemate, then, does not refer to pluralist legislative deadlock but to economic policy from the class perspective: "The state's fiscal capacities must be used as the catalyst of capital accumulation (growth), but there must be no progressive income redistribution and no controls over investment decisions. The latter strictures, dictated by national political alignments, make the former goal unreachable. This is the stalemate that has attended the making of economic policy from Kennedy to Reagan."

Within each simplified theoretical perspective, Winant sticks to fairly familiar ground, though he sometimes overstates his case. Elite theorists will agree that presidential initiatives do sometimes demonstrate an elite politics of technical rationality. Insider accounts show, however, that the "comprehensive" plans of presidents are often as much a product of particular bureaucratic interests scrambling wildly for their narrow goals as of objective planning. Pluralists will concur that congressional action is largely pluralist, though no doubt many members of Congress saw Chairman Mills's ability to control committee votes and floor rules as less than open. The pluralist and elite sections are largely empirical, though; and Winant on the whole makes a good case for his position. The same cannot be said for the parts using class analysis. Here Winant relies more on imperatives (e.g., "Fiscal capacities must be used") than evidence. Capitalist economies may be prone to stagnation and crisis, though Russia and Japan might hint that these tendencies are neither exclusive nor absolute. Winant accepts the logic that the tax cuts must subsidize wealth and investment while squeezing consumption, for example, but fails to show that the cuts actually did either of these things. Others seriously doubt that they did. John Witte, in a book that all who write about tax policy should read, argues that the 1964 Revenue Act was in fact modestly progressive (*The Politics and Development of the Federal*

Income Tax [1985], p. 165).

But the strong point of Winant's book involves his theoretical synthesis, not his discussion of the individual theoretical perspectives. He asks not, "What theory is correct?" but "What is correct in each theory?" In this the book is closely related to Robert Alford (one of Winant's mentors) and Roger Friedland's *Powers of Theory* (1985). Instead of stopping with theory, as they do, he attempts to apply it to actual historical events. This is not an easy job, and this synthesis comes at rhetorical price. Mixing the language of "pork fat" and "decommodification" in the same volume will probably satisfy neither Gramsci nor Polsby. For those with a theoretically open mind, this book will both educate and frustrate.

MARK ROM

University of Wisconsin, Madison

COMPARATIVE POLITICS

Soviet Ukrainian Dissent: A Study of Political Alienation. By Jaroslaw Bilocerkowycz. Boulder: Westview, 1988. 280p. $27.50.

This is a successful first attempt to analyze Soviet Ukrainian dissent from 1963 to 1981 in terms of political alienation. The author builds on the concept of alienation developed by David Schwartz, who speaks of the core meaning of separation or estrangement. That Bilocerkowycz applies the theory of a fellow political scientist rather than trying to invent his own is a recommendation rather than a criticism. In dealing with persons who have deliberately challenged the Soviet system, the concept of political alienation, that is, of people's no longer identifying with the system, is useful.

Who are the Ukrainian dissenters? What do they want? What are their chances of succeeding? Kenneth C. Farmer, in his *Ukrainian Nationalism in the Post-Stalin Era* (1980), analyzes a maximum of 139 dissenters (pp. 176–84). In his more broadly gauged book, Bohdan Krawchenko identifies and briefly analyzes as many as 942 Ukrainian dissenters (*Social Change and National Consciousness in Twentieth Century Ukraine* [1985], pp. 250–53). Bilocerkowycz has worked with a larger data base than Farmer, but he has wisely decided to define a dissident more narrowly than Krawchenko. For Bilocerkowycz, a "Ukrainian dissident" is a person of Ukrainian nationality "who expresses political disaffection through some overt behavioral action and is subsequently subjected to punitive sanctions, usually involving arrest and penal incarceration" (p. 9). Bilocerkowycz has collected biographical data for 210 such dissenters, and he subjects his sample to intensive qualitative and quantitative analysis. An admirable feature of Bilocerkowycz's book is his operational precision: for instance, all the members of his sample are listed by name (pp. 219–20).

Bilocerkowycz's composite picture of dissenters is noteworthy: "Ukrainian dissidents, in most respects, are sociodemographically atypical of the Soviet Ukrainian population. The dissidents were exceptional achievers in Soviet society as measured by their high level of education, literary or arts and humanities-based occupational background, social class mobility (rising from peasant origins to the intelligentsia), and Communist Party membership. They were more often male, natives of Western Ukraine and of rural areas, middle-aged, and educated in a VUZ [higher educational institution] located in Kiev or L'viv *oblasts* than was the general population" (p. 130).

The dissenters have clashed with the authorities in that they have advocated "self-determination versus subordinate republic status; linguistic, cultural, and ethnic pluralism versus Russification and assimilation; religious freedom versus religious persecution and atheism; democratic rights versus authoritarian restrictions; rule of law versus subordination of law to politics; expanded economic rights and balanced development versus limited economic rights and unbalanced development; social and national equality versus inequality and discrimination" (pp. 54–65). The author is excellent in soberly cataloging the regime's punitive response to dissidents (pp. 160–82).

In his concluding chapter Bilocerkowycz gives a prudent, qualified answer to the question about the dissenters' ultimate success. On the one hand, Gorbachev appears rather insensitive to the legitimate concerns of non-Russians, as many Ukrainians, Uzbeks, Kazakhs, and—more recently—Armenians will attest (p. 195). On the other hand, in 1986-87 Ukrainian establishment literati have used Gorbachev's *glasnost* to revive some of the cultural and religious demands of Ukrainian dissenters (pp. 196–98).

I have a reservation about the author's almost peremptory exclusion of non-Ukrainian dissidents residing in the Ukraine, such as Russians and Jews (pleading lack of space, p. 14, n. 54). While the majority of such dissenters may oppose many demands of the ethnic Ukrainian dissenters, there are notable exceptions, whose significance should have been explored. For instance, the Russian Vladimir Malinkovich, of Kiev, did join the Ukrainian Helsinki Group; and the working relationship between the Ukrainian and the Moscow Helsinki Group was good. The Ukrainian Helsinki Group also included as member Iosyf Zisels, a Jew (p. 81). To really complicate things, Leonid Pliushch, who is listed as one of the 210, until the mid-1960s considered himself a Russian (p. 143). He married Tatiana Zhytnykova, who is Jewish and who eloquently defends Ukrainian patriots (Farmer, pp. 168–69).

Copyediting has not always been perfect. The name of the Polish leader is Gierek, not Geirek (p. 85); and the year of a source is 1979, not 1977 (p. 85, n. 45). There are several conspicuous typos as well.

Those minor shortcomings notwithstanding, Jaroslaw Bilocerkowycz's book is a superb, concise but well documented analysis of Ukrainian dissenters from 1963 to 1981. It is well written in a crisp style.

YAROSLAV BILINSKY

University of Delaware

Palestinians in the Arab World: Institution Building and the Search for State. By Laurie A. Brand. New York: Columbia University Press, 1988. 320p. $35.00.

Whatever the ultimate outcome of Palestinian demands for self-determination and an independent state, Palestinians clearly evince a strong sense of political community. In the occupied territories, it is precisely such identity and cohesion that sustain the present Palestinian *intifada* (uprising). In the Palestinian diaspora, a complex and overlapping network of social and organizational linkages has developed to support Palestinian communities dispersed and displaced since 1948 or 1967.

It is this second aspect of Palestinian sociopolitical organization that represents the focus of Laurie Brand's book. In it she examines the development of five of the most important Palestinian popular organizations (the General Union of Palestine Students, the General Union of Palestine Workers, the General Union of Palestinian Women, the General Union of Palestinian Teachers, and the Palestine Red Crescent Society) in Egypt, Kuwait, and Jordan. Whether reconstructed from the remnants of pre-1948 Palestinian institutions or organized as a response to the post-1948 diaspora, such organizations were an important part of the gradual resurgence of Palestinian sociopolitical organization through the 1950s and 1960s. By the 1970s they had become an integral element of the broader Palestinian nationalist movement represented by the revitalized Palestine Liberation Organization. As such, they have provided a concrete linkage between the political movement and the mass population, offered services to local Palestinian populations, and constituted an important arena for political competition between the dominant ideological trends within the Palestinian movement.

In the past, the development of the sociopolitical bases and institutions of the modern Palestinian movement has been largely neglected by Western (if not Arab) analysis, which has tended to focus instead on the diplomatic-military activities of the PLO. Brand's study would thus be of value even if it were simply a political history of Palestinian popular organization in the diaspora. It is, however, much more than this.

Palestinians in the Arab World is an important theoretical and methodological advance in the study of Palestinian politics and political development. Brand has sited her study within a theoretical framework derived from Charles Tilly's model of political mobilization. Accordingly, she focuses on the motivations and rationales behind political activity, structures and organizations, and opportunities for political mobilization. She proceeds to argue that "the most crucial of these elements is that of opportunity" (p. 18). Palestinian political mobilization has been shaped by varying degrees of host regime tolerance, constraint, and coercion. Brand suggests that a "regime's willingness to exercise repression is based on the degree of economic or political marginality of the Palestinian community to the country's productive structure" (p. 229). Greater marginalization (as in Egypt) renders the Palestinian community less of a threat, and a regime is likely to be more tolerant of community organization. With integration (as in Jordan) the threat potential of Palestinian organization increases, and regime tolerance correspondingly declines.

Methodologically, Brand's contribution lies in the way she has accessed and integrated published and unpublished Arabic materials with extensive research interviews. The interviews are particularly impressive (especially considering the difficulties of this type of research), including discussions with scores of participants ranging from local Palestinian union activists to members of the PLO executive committee. Their testimony is carefully and effectively used; the data and perspective brought to bear are of exceptional value.

Both methodology and the analytical framework adopted by the book do, however, tend to lead Brand away from examining one of the more interesting questions of Palestinian popular organization: the relationship between Palestinian institutions, the various Palestinian political-military *tanzimat* (organizations), and the PLO. The book notes, but only partially explores, the charges of bureaucratization, dependency, and the subordination of union activities to inter*tanzim* competition that have been increasingly raised within the Palestinian movement. This is hardly surprising. Brand, after all, is primarily interested in relations with host regimes, and interviewees were doubtless more forthcoming on questions of early organizational development than on the contemporary dynamics of intra-Palestinian politics. A fuller examination of the activities of the PLO's Department of Popular Organization, interviews with leadership figures from the major *tanzimat,* and discussion of the dynamics of leadership selection in mass organizations (whether preapportioned through inter*tanzim* negotiation or selected through competitive or noncompetitive electoral slates) might have allowed the author to get at these issues and to tie them into the broader political and policy dynamics of the Palestinian movement as a whole.

This, however, is more an appeal for the application of Brand's analytical insight to related questions than a critique of approach or content of her present study. *Palestinians in the Arab World* is a very fine book and a significant contribution to our understanding of the contemporary Palestinian movement.

REX BRYNEN

McGill University

The Social Origins of Political Regionalism: France, 1849–1981. By William Brustein. Berkeley: University of California Press, 1988. 224p. $28.00.

Political regionalism is a term generally used to describe a pattern of political involvement that is strongly associated with a regional base. Until recently, political regionalism has either been studied from a normative point of view, emphasizing the transmission of values and norms from one generation to the next as the principle determinant of political preferences or examined through a structural perspective, asserting that durable social structures are the principle galvanizer of voting choices.

In this work, Brustein presents a unique approach, wedding rational choice assumptions concerning individual behavior with the Marxist concept of the mode of production. He argues that the persistence of distinctive regional political behavior in France is based on the existence of discrete regional modes of production. These regional social structures produce specific constellations of interests among cultivators and in turn result in patterns of aggregate political behavior. Brustein then tests this mode of production theory empirically on archival data collected for three key

French national legislative elections, those of 1849, 1914, and 1981 (pp. 1-5).

The strengths of this book can be found in its first three parts. Here Brustein elucidates a unique and unusual theory of political regionalism, presents an in-depth survey of the key regional modes of production that have evolved in France since 1789, and considers the meaning of the Left and the Right in France as it relates to the interests of French cultivators. The French archival data is new and extensive, and the historical analysis is invaluable to both student and expert alike.

The weakest part of the book is Brustein's attempt to test hypotheses derived from the mode-of-production model. He argues that it is necessary to analyze elections from periods in French history when the central issues separating the Left and the Right were at their most extreme. Surprisingly, Brustein never thoroughly discusses the elections of 1936, which signaled the rise of the left-wing coalition of the Popular Front or the elections following the student demonstrations in May 1968. Both of these national elections would seem to be representative of central issues separating the Left and the Right and important to consider in the testing of the model.

While Brustein may not be entirely successful in testing his model, he does present an innovative theory of political regionalism. His use of new archival data and his discussion of modes of production in western and Mediterranean France should be important to French historians as well as to anyone interested in questions of regional political behavior, past and present.

KAREN J. VOGEL

Hamline University

Psychoses of Power: African Personal Dictatorships. By Samuel Decalo. Boulder: Westview, 1989. 232p. $29.95.

Through three case histories from recent African history, Decalo explores personal abuse of state power, focusing on the psychological dimension within the context of social, political, and economic development. Decalo presents detailed analysis of three tyrannical regimes and their megalomaniac rulers: Idi Amin of Uganda, Jean-Bedel Bokassa of the Central African Republic, and Francisco Macias Nguema of Equatorial Guinea. According to Decalo, "Personal dictatorship is a unique form of autocracy at the tyrannical extreme along the spectrum of authoritarianism, characterized by the personal, aberrant, and tyrannical nature of the despotic power exercised" (p. 185). Although these three cases are hardly representative either of all African rulers or of all authoritarian rulers, they do illustrate what can happen when an aberrant personality coincides with personalist rule. Decalo seeks to answer three questions: How do these types of personalities come to power? What characterizes their rule? and How do nations recover from their rule? While *Psychoses of Power* dwells on extremely negative worst-case scenarios, Decalo asserts that it is just such cases that can sensitize us to developing checks on absolutist power structures and tyrannical personalities.

There is a good case to be made that Amin, Bokassa, and Nguema were particularly brutal dictators. As a lecturer at Makerere University for four years during Amin's regime, I can testify to the capriciousness and brutality that characterized Uganda in the 1970s. Decision by decree and dream substituted for careful deliberation and cabinet government. In the absence of regular and public debate over policy, the Amin regime announced policies and personnel decisions on the radio. Decalo describes the Bokassa and Nguema regimes as similarly dependent on the whims of a personalist ruler.

For each of the three cases, Decalo gives a detailed social, economic, and political history. Against these structural factors, Decalo presents the rise of the aberrant personality. In each case, Decalo indicts colonial and neocolonial powers for supporting the advancement marginal functionaries who had displayed promotability based on ruthlessness, not rational competence. Once the colonial powers had elevated these characters to high-level positions, they became available as national leaders when sociopoliticoeconomic conditions worsened. Thus, Decalo reminds us to pay attention to recruiting within public service systems as well as to reducing social stresses and increasing economic prosperity. I would add that educational and socialization institutions must be developed to select for leadership ladders people with an aptitude for rational deliberation, cooperation, and respect for humanity.

Decalo's worst-case scenarios are fascinating in their detailed presentation and analysis of "the systemic brutalizations of their populations, the utter pulverization of the societal fabric of Uganda and Equatorial Guinea, and the fundamental socioeconomic dislocations in the Central African Republic" (p. 182). Decalo recounts early personal histories of marginal, seriously malintegrated, and force-oriented personalities. Despite their negative characteristics, which were clearly recognized by internal and external political forces, these personalities were ready and willing to assume power when the opportunity arose.

Influenced by soothsayers, Amin seized power during a period of growing economic malaise, on-going political strife, and likely threat to Amin's status. His regime resulted in widespread interethnic violence and an emphasis on military hardware at the expense of social services, including education. Heavily influenced by delusions of grandeur, Bokassa seized power during a period of progressive economic decay, erosion of political authority, and competing bids for national control. While maintaining most popular social services, the Bokassa regime maintained the economic services in order to extract resources for his imperial extravagances. Nguema seized power in Equatorial Guinea during protracted internal and neocolonial struggles. Once triumphant, Nguema, heavily influenced by witchcraft, decimated the once-healthy, but externally controlled, economy through the systematic disintegration of the government policies and decision-making infrastructure. All three rulers bequeathed politicoeconomic systems so devastated that reconstruction in the 1980s has been slow, if not impossible.

Although *Psychoses of Power* aims to contribute to the literature of political psychology and psychodynamics of leadership, Decalo appropriately includes equal treatment of the environmental influences on leadership change. For those seeking prescriptions for improving leadership recruitment, arm-chair psychological studies provide few formulae for "fixing people," especially those least willing to seek expert assistance for self-improvement. On the other hand, Decalo's analysis of the personal and social backgrounds of three tyrants reminds us that early childhood institutions are critical for national development, as are strong educational systems and strong evaluative mechanisms in public administration.

Both from the academic perspective of pluralistic democracy and from the personal perspective of living under one of these regimes, an important aspect of Decalo's analysis is the emphasis on personalist rule, accompanied by brutality and terrorism. In these three cases personal authoritarianism went unchecked because alternative decision-making institutions were emaciated, often through deliberate killing or intimidation of educated experts. All three leaders were relatively uneducated and apparently suffering inferiority complexes, especially in relation to educated expertise. Social, political, or economic stress can result in conditions favorable for the seizure of power by military or authoritarian leaders whose policies may still be developmental and whose processes may still be pluralistic. Decalo's analysis reminds us that societies must ensure that leadership promotion in all institutions must reward intelligence, cooperative spirit, and respect for humanity.

JEANNE-MARIE COL

Sangamon State University

The Soviet Union through French Eyes, 1945-1985. Robert Desjardins. New York: St. Martin's, 1988. 208p. $45.00.

This essay, written by a French Canadian who studied Soviet politics with Archie Brown at Oxford, is a concise and illuminating commentary on French studies of the Soviet Union from World War II until the eve of the Gorbachev era. The author's stated purpose is to provide a guide to the work of French Sovietologists for their English-speaking counterparts.

To this end Robert Desjardins focuses on the three major schools of French Sovietology: the first grounded in the Marxist tradition, the second committed to variants of the totalitarian paradigm so common in Anglo-American writing on the Soviet Union during the 1950s, and the third engaged in "institutional analyses." The Marxists, whether acritical admirers of "real, existing socialism" during the

Brezhnev years or Trotskyists dismayed by what they saw as Stalin's betrayal of the Bolshevik heritage, have one thing in common. They are convinced of the Soviet Union's singularity, uniqueness, and unchangeability (except through yet another revolution, in the Trotskyist view). The adherents of the totalitarian model, represented by Raymond Aron but increasingly prominent since the mid-1970s, are not so very far apart from the Marxists. They, too, are convinced of the ideologically conditioned exceptionalism and durability of Soviet totalitarianism. The institutional analysts come closest to contemporary Anglo-American students of comparative politics. Exemplified most notably by Hélène Carrère d'Encausse and Michel Tatu, they joined English-speaking Sovietologists in shifting from the isolationism of area studies to the application of at least some of the analytical tools of comparative politics to conduct research on the Soviet Union in the post-Stalin period.

One of the author's valuable contributions is the light he sheds on why this third group has had such a small following in France, notwithstanding the considerable respect its members enjoy abroad. Their influence at home has been circumscribed by the structural obstacles posed by the French educational system to area studies, systematic comparative politics, and interdisciplinary research. It has likewise been limited by the cultural hegemony of Marxism and the French Communist party in the immediate postwar years and the subsequent political polarization of French society. In short, much French writing on Soviet affairs has tended to be partisan.

While Moscow's suppression of the Hungarian Revolution in 1956 had the effect of breaking the left-wing taboo against French scholarship on Soviet society, it also gave rise to a right-wing preoccupation with portraying the Soviet Union as the epitome of totalitarianism. As Desjardins documents, French conservatives claimed that the Soviet form of totalitarianism emanated from Marxist-Leninist ideological precepts rather than developmental exigencies. They interpreted the Stalinist system as an ideologically prescribed and self-perpetuating polity rather than as an aberrant transitional stage in the modernization process. To their way of thinking, moreover, the Soviet Union was as unamenable to comparison with non-Communist societies as it was immune to evolutionary modification. In the words of Alain Besançon, disenchanted ex-communist and, according to Desjardins, paterfamilias of the right-wing tendency in French Sovietology, it is only by "resolutely forgoing the 'comparatist' approach that one may hope to comprehend the Soviet phenomenon" (p. 80).

Desjardins neglects to comment on one important point, however. In recent years the Marxist and the "totalitarian" schools of French Sovietology converged with the institutional, or comparative, analysts in their shared disregard for the possibility, let alone the likelihood, of meaningful change in the Soviet Union. The author describes Carrère d'Encausse, for example, as one whose "voice may be added to those which have already expressed pessimism with respect to what may be expected to emerge from the new Gorbachev period" (p. 111). This blindspot may be due to the fact that the comparativists, for all their familiarity with Anglo-American political science, did not emulate the "change to change" undertaken by their English-speaking colleagues over the past few decades. It was this shift that enabled the more daring and innovative Sovietologists among them (Jerry Hough, for example) to examine the Soviet Union from the perspective of change rather than continuity.

Desjardins has done a fine job of synthesizing the vast body of French Sovietology for English-speaking scholars in the field. He has not, however, succeeded in disproving a widely held view that French study of the Soviet Union suffers from a pervasive state of underdevelopment. The French institutional analysts may have the analytical tools to explain, with the benefit of hindsight, the kaleidoscopic changes enveloping the USSR as we enter the 1990s. But the scholars of the Marxist and "totalitarian" schools, who rejected the prospect of substantial change in the Soviet Union as a matter of intellectual conviction, are caught in a mental straight-jacket of their own making. They are in the unenviable position of being conceptually unprepared to explain the contemporary transformation of Soviet society even after the fact.

JOAN BARTHE URBAN

Catholic University of America

Coalition Strategies of Marxist Parties. Edited by Trond Gilberg. Durham, NC: Duke University Press, 1989. 379p. $52.50.

The appearance of a work devoted to this topic could not be more fortuitously timely. Presently, many civil wars in the Third World are inching to the stage of political resolution where the issue of coalition of the communist forces or the contending communist forces with their rivals is squarely on the agenda. Whether it be the case of Kampuchea or Nicaragua, the coalition issue is a salient one. As a result, an exploration of the vagaries of Marxist parties' approaches in Europe and other continents to political coalitions is most useful.

The essays gathered here present these chronicles of coalition politics in mainly historical fashion for their individual areas, Eastern and Western Europe, Latin America, Africa, India, Indonesia to 1965, and Vietnam without expressly drawing theoretical points from their analysis. But both the editor and Jiri Valenta contribute insightful essays, noting that the issue of coalition politics affects Marxist parties in a profoundly unique manner because for them the existing political system, within which they must contend for power, is regarded as being ab ovo illegitimate. As Gilberg, in his essay on Western Europe (which commendably includes Scandinavia) and Valenta indicate, a party committed to a revolutionary project and a monopoly of power is inherently suspicious of coalition and equally suspected by its potential partners. Many of the considerations of the theory of coalition developed by Riker and colleagues do not apply to anywhere the same degree here because the extreme tactical flexibility of these parties is juxtaposed with the abiding sense of their or the coalition's illegitimacy. For them the issue is not how or when to coalesce but whether to make a coalition. In effect Leninism, though it offers strength in organization and the support of the USSR, also inhibits Western parties from effective participation in Western polities. So, too, their relation to the USSR both offers novel possibilities and limits them at the same time in the Third World.

Charles Gati's penetrating essay on postwar Eastern Europe makes clear that the Soviet armed presence after 1944 made a fundamental difference in enabling coalitions to go forward and led to monopolistic polities by 1947–48. But at the same time this outcome was not necessarily foreordained for any one country, let alone the entire region, and depended on both local and international trends. He explicitly relates those developments of 1944–48 to the issue of Afghanistan and other civil wars today (p. 108) and concludes both that Communist parties are difficult, if not duplicitous, partners and that their victory is not inevitable in coalitions. Indeed, such victories depend on favorable conjunctures like the presence of the Red Army and the relative skill of the coalitions' partners.

The contributors find many of these variables equally present in the Third World. Stanley Kochanek finds many of the difficulties experienced by the Indian Communist parties to lie in the ideological and sectarian nature of their parties, which makes coalition decisions matters of protracted and intense soul searching (pp. 205, 209). Frank Cibulka relates the difficulties the Communist party of Indonesia had in Indonesia under Sukarno to the authoritarian and undeveloped political structures of Third World polities that inhibit opportunities for more expansive and open coalition building and politics. At the same time the Soviet presence and habit of using, and then discarding, Third World parties often saddles them with the perception of being fifth columns for Moscow or else pawns to be sacrificed on its altar of realpolitik. These two dangers have happened so often and pervasively across Africa, as David Albright's detailed study shows, as to make coalition politics at home inherently perilous. Accordingly, as he indicates, many parties have opted for "secondary collaborators" like the Front Line states' coalition against apartheid rather than accepting too close an embrace with the USSR.

Finally, in both his introductory and concluding essays the editor relates the issue of coalition to the fundamental one of socialist internationalism. At a time when radical parties publicly question Soviet directives and stress instead the campaign for peace and new thinking—a new mass coalition or united front strategy—rather than revolution and the civil wars are reaching the acute stage, this point does, indeed, become a critical one. The fact that the book raises so many points of both topical and theoretical value augurs well for its continuing relevance for some time to come.

STEPHEN J. BLANK

Air University

Searching for Rural Development: Labor Migration and Employment in Mexico. By Merilee S. Grindle. Ithaca: Cornell University Press, 1988. 216p. $24.95.

This book is an impressive contribution to the literature on Mexican-U.S. migration. It thoroughly examines the relationship between external labor migration and the local agricultural economy. Anyone concerned with solutions to this problem will find the book insightful. Most of the literature on this topic is descriptive and analytical, but not normative. The book has an exhaustive review of other work and displays an objective, nonpolemical approach. One comes away with some optimism on a topic usually soaked with pessimism.

The fundamental theme of the book is that Mexican village development should rely on a judicious mix of U.S. migration, migration within Mexico, local agricultural development, and local industrialization. Grindle argues that the Johnston-Mellor strategy, in which agricultural development simultaneously fuels food self-sufficiency, export earnings, and urban growth, is unrealistic as the sole solution in a country where the biggest need is for rural employment opportunities and where the prerequisites for commercial agriculture do not exist. The author investigates several alternative rural development strategies (government programs such as the Program for Integrated Rural Development and the Mexican Food System, altered *ejido* structure, rural industrialization) and finds them lacking, owing primarily to political inefficiency and inertia. Her conclusion from this review is that migrant remittance income may provide an important complementary income source and a source for local investments to make the community more self-sufficient in the long run. Finally, Grindle finds traditional economic models (Lewis's, Todaro's, dependency) inadequate to explain the enduring, cyclical nature of Mexican migration to the United States.

The best chapter of the book, chapter 5, assesses the history and impacts of U.S. and internal migration on four rural *municipios* in central Mexico (Tepoztlan, Morelos; Jaral del Progreso, Guanajuato; Union de San Antonio, Jalisco; and Villamar, Michoacan). These four *municipios* form a continuum regarding the role of U.S. migration. Tepoztlan is an exurban economy tied to tourists, summer homeowners, and industries in Mexico City and Cuernavaca and depends only marginally on U.S. migration. Jaral del Progreso is a dynamic agricultural economy in the Bajio with a large underclass dependent on U.S. migration for survival and exhibits a moderate dependence on U.S. migration, as does Union de San Antonio, where dairy operations provide only partial support to the town's prosperity. Finally, Villamar exhibits an almost complete dependence on U.S. migration even in the presence of considerable local agricultural potential and epitomizes many of the tenets of the migrant syndrome described by *dependencistas.* The latter part of the book outlines the fundamental needs of the agricultural sector in Mexico and the relationship between Mexican rural development and U.S. ties in the form of trade, immigration, and investment. This section is only peripherally tied into the findings of the case studies.

As a geographer whose research centers on the topic of patterns of undocumented migration from Mexico to the United States, I have found much of use in this book; and I strongly agree with Grindle's arguments and conclusions. My criticisms are more structural than substantive. I found the writing style just a bit stodgy, with an inexplicable avoidance of well-accepted terms and phrases. For example, why not use *migrant selectivity* instead of *constraints on portfolio options* (chap. 2) or *dependency theory* instead of *the dynamics of capitalist accumulation* (chap. 2) or terms such as *channelized migration, shuttle migration,* and *social overhead capital,* instead of more unwieldy constructs? A second criticism is that too few data are presented to enable the reader to judge, for example, whether the demographic-economic profile of Tepoztlan is typical of rural Mexico or not or to gauge the relative importance of U.S. versus domestic migration for Union de San Antonio. In fact, practically no explanation of the survey design for the 1985 village interviews is given. Finally, the last two chapters are somewhat of a disappointment in that they do not present ultimate suggested solutions tied to the case studies—for example, migrant-village cooperatives in the FEDC mold and immigration and trade policies targeted to specific sending *municipios* (e.g., via a renovated guest worker program).

Overall, this book is an important scholarly contribution to the literature on international migration between Mexico and the United States. It fills a niche not filled by existing books and marks a solutions- and future-oriented approach that is innovative and refreshing.

RICHARD C. JONES

University of Texas, San Antonio

Resistance and Integration: Peronism and the Argentine Working Class, 1946-1976. By Daniel James. Cambridge: Cambridge University Press, 1988. 300p. $42.50.

Daniel James has produced a useful, sophisticated book on a crucial issue dealing with contemporary Argentina. Building on past work on Peronist labor and the Peronist Left, the book constitutes a major achievement within space and topic limitations. The author attempts to explore and explain the relationships between the Argentine working class and Peronism from 1946 to 1976, but the book's most valuable contribution is for the 1955–73 period. The 1973–76 years are covered summarily (pp. 242–48) and do not provide the reader with the analysis and insights of previous sections. For instance, Lorenzo Miguel, a key trade union leader, and the current he represented during Isabel Perón's administration are hardly covered in the text.

The book has two closely connected main themes. The first is the relationship between Peronist union leaders and rank-and-file members, including the development of a particularly resourceful union bureaucracy *(burocracia sindical)*. The second deals with the impact of Peronist ideology on the Argentine working class and why the latter has remained strongly attached to the movement originally led by Juan Perón (1895–1974).

James does not simply employ a narrative analytical approach in his work. He also deals with the concrete experience of Peronist workers in the renewed tradition of current labor history to which he rightly belongs. His sources are varied, from standard literature in Spanish and English to "unofficial Peronist newspapers, rank-and-file newspapers, pamphlets and barrio broad-sheets" (p. 4), besides a number of interviews and discussions with participants in the period's union activities. The book is refreshingly free of misprints and factual errors.

These comprehensive resources enable the author to avoid mythologizing or trivializing his topic by revealing the faces behind the masks. After a synthetic consideration of the formative relations between organized workers and the Peronist state and the ambivalence of the historical legacy from the 1940s and 1950s, James develops his view of a dialectically combined series of tensions between resistance to, and integration with, the status quo, of a "shop-floor culture" (p. 58) and the "institutional pragmatism" (p. 130) of labor leaders like Augusto Vandor. What James uncovers is a process fraught with ambiguities, even contradictions, where Peronism as a social movement did not always coincide with "its functional needs as a specific form of state power" (p. 40). Some of the author's arguments on the partially functional role of Peronist union bureaucracies continue the analysis of Ducatenzeiler's *Syndicats et politique en Argentine, 1955–1973* (1980), and James' convincing notion of clashes between conciliatory and heretic trends within Peronism carries several steps forward the stimulating view of Corradi's "Between Corporatism and Insurgency" (in *Terms of Conflict*, ed. Morris J. Blackman and Ronald G. Hellman [1977]).

The author's conclusions are sound and appropriate as a background to Argentina's economic exhaustion and democratic transition by the late 1980s. James emphasizes both the unusual dynamism, organizational creativity, and resistance against political repression manifested by the Argentine working class and the parallel need to achieve, even temporarily, an integration with the status quo, according to different circumstances and experiences. He sees in this tension one of the key original features of Peronism that accounts for its longevity and relevance as an ideology. It is also possible to suggest that a combination of strong unions and a (generally) weak Peronist

(Justicialista) party can become a serious problem in the consolidation, not yet fully established, of liberal democracy in Argentina. It is in this area that one misses a fuller discussion by James of authoritarian-corporatist and democratizing trends *within* working class organizations as another important feature of Peronism as both a movement and an ideology.

ALBERTO CIRIA

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The Chinese Army after Mao. By Ellis Joffe. Cambridge: Harvard University Press, 1987. 224p. $22.50.

The Chinese Army after Mao is the first book-length effort to analyze the transformation of the Chinese armed forces over the past decade. Joffe regards the post-Mao period as one of far-reaching accomplishment. In his view, the military leadership has largely recovered from the trauma of the Cultural Revolution and has returned to its more delimited professional obligations. He further asserts that the People's Liberation Army (PLA) has made substantial headway in building a credible, more modern fighting force.

Joffe contests the view that the PLA has been a principal loser in the post-Mao reforms and that many leaders are therefore disaffected from recent political and economic changes. The armed forces, he asserts, are "one of the main beneficiaries" of the reform process, with the PLA within "the mainstream of China's 'second revolution'" (p. 33). Joffe concludes that there is a substantial consensus within the high command on the need for military modernization, with resistance consigned principally to those retaining a personal or ideological stake in the army's politicization of the 1960s and 1970s. In essence, he believes that the PLA has resumed a process of professionalization initiated in the 1950s but seriously disrupted during two decades of political upheaval.

Joffe reviews changes over the past decade on six major issues: perceptions of external threat, trends in weapons acquisition and defense spending, military doctrine, defense industrial capabilities, institutional development, and civil-military relations. Although he concedes that there have been periodic leadership conflicts on some of these questions, he argues that Deng Xiaoping's dominance over the political process has guaranteed broad assent within the military to various policy changes. In Joffe's estimation, the changes have been especially marked in perceived threats to Chinese security, revisions in military doctrine, and in the enhancement of professionalism within the officer corps.

At the same time, he contends that the PLA has fundamentally recast its approach to defense planning. Mao's supposed belief in people's war "has been almost completely abandoned" (p. 93), supplanted by a more realistic doctrine of deterrence at both the conventional and nuclear level. In Joffe's view, China's measured, incremental approach to military modernization has entailed "a sustained effort to raise the combat capability of the PLA through a slow improvement of technology and a sweeping improvement of the nontechnological components of military power" (p. 182). Although he avoids definitive conclusions, Joffe seems convinced that the Chinese are making real progress in building a modern, professionalized defense force.

Joffe argues his case with considerable conviction and admirable conceptual clarity. He also writes extremely well, with none of the befuddling jargon that afflicts much of the literature on civil-military relations. But an uneasy sense lingers that the story is much more complicated than he conveys. There are few rough edges in Joffe's account. He prefers to take the Chinese at their word. He asserts that "Deng [has] completed the process of removing the PLA from politics . . . without serious and disruptive struggles, or even visible tensions, between the civilian and military leaderships" (p. 162). However, it strains credulity to believe that those senior officers comfortably ensconced at the top of the Chinese system after Mao's death have gone gently into the night. Indeed, the evidence points in the contrary direction: it has taken Deng a very long time to displace those associated with the policies of the past.

Unlike Mao, however, Deng has rarely opted to humiliate those he judged unequal to the task publicly. As a result, military politics under Deng lacks the extremely personalized, almost nihilistic quality of the Cultural Revolution era. The stubborn (and successful) resistance of senior commanders to Deng's prolonged effort to elevate Hu Yaobang to chairmanship of the Party Central Military Commission underscores that the PLA has not always proven pliant to Deng's preferences. Similarly, Deng's retention of this post beyond the Thirteenth Party Congress of late 1987 bespeaks his recognition of unfinished business.

The study's principal limitation, however, is methodological. Joffe regularly cites numerous Chinese claims about major changes in operational procedures, training, and weaponry; but he never scrutinizes them. He offers no independent assessment of the credibility of these assertions, preferring to accept them uncritically. Such declaratory changes seem more important for the political message they impart than for their manifest content. In essence, they are closer to public relations than to demonstrable accomplishments. Modernization is definitely on the military policy agenda, but there has been no magical institutional transformation in a very brief period of time. This judgment pertains most critically to Joffe's discussion of combined arms operations, a long-term goal for which Chinese commanders devoutly wish; but that has remained in embryonic form for most of the 1980s.

There is also an excessively forced quality to the critique of Mao's role in military policy. Joffe asserts that China's ranking military leaders have been singularly intent on dismantling Mao's approach to military doctrine, posing the late chairman as a revolutionary romantic incapable of seeing beyond the glories of China's guerrilla traditions. Yet there is cumulative and compelling evidence of Mao's towering prestige as a military strategist among his comrades-in-arms, even if many of them are also highly critical of Mao's embroiling the armed forces in two decades of divisive internal political conflict.

Joffe also seems noticeably circumspect in addressing the more personalized, less issue-oriented factors that influence all Chinese institutions. For example, the ultimate readiness of numerous senior military leaders to relinquish their posts has frequently been correlated with the appointment of their children (and in some cases, their children's spouses) to ranking positions within the military apparatus. Many of this younger generation may indeed be more technically competent, but to ignore such familial considerations is to lose sight of a profoundly important characteristic of the military system. In addition, Joffe addresses only in passing China's increased involvement in the international arms market and the implications for those within the military who stand to gain by such transactions. Here, as well, there is a much larger story to be told.

On balance, there is a period-piece quality to this book. Slowly but perceptibly, a much wider array of evidence is becoming available to scholars of Chinese military affairs. The official version of events remains the necessary starting point in analysis, but it is now possible to move beyond it. The study that explores more satisfactorily the wrenching, still tentative movement of the Chinese armed forces away from two decades of upheaval remains to be written.

JONATHAN D. POLLACK

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Economics and Elections: The Major Western Democracies. By Michael S. Lewis-Beck. Ann Arbor: University of Michigan Press, 1988. 256p. $34.50.

Over the last few years, the study of the economy and voting has shown encouraging signs of becoming a theoretical subject. After a decade (the 1970s) of experimenting with new techniques and searching for new indicators, there is now both a broad consensus on the structure of basic models of the economy's impact on voters and a commitment to extending these models at higher levels of rigor, abstraction, and formalization. Most important, extensions are increasingly sought to generalize models capable of finding the same things happening in different places and at different times rather than just explaining special cases.

Accordingly, the central purpose of Lewis-Beck's book is to provide a rigorous, carefully controlled, quantitative comparative study of some central empirical elements of political economy. His major claim is that in Europe personal "pocketbook" voting—known to be weak in the United States—is more or less nonexistent. In Europe, the impact of the economy on electoral behavior is channeled instead through more complex or mediated perceptions of the impact of government policies on one's personal financial condition, both retrospective and prospective evaluations of national or collective economic performance, and anger over government's handling of the economy. Similar economic influences appear in voting in all his cases (Britain, Germany, France, Italy, and Spain) even after the effects of social class, ideology, and religion are controlled. However, the economy-vote relationships are generally stronger in Britain and Germany than in France and Italy, which Lewis-Beck attributes to greater economic growth in the first two and more complex coalitional government (diffusing attribution of responsibility) in the latter.

The book is the natural comparative politics companion to Rod Kiewiet's *Macroeconomics and Micropolitics* (1983). It offers a systematic and rigorous exploitation of a unique data base. Based on survey materials incorporated in several Eurobarometers, Lewis-Beck's cross-sectional indicators allow contrasts of the effects of personal and collective, past and future, and cognitive and affective aspects of economic policy evaluations by members of the public. His discussion of statistical methods is thorough. Much detail is given about controls and alternative specifications. Sources are carefully reviewed. The creation of comparative survey instruments and the care taken over maintaining comparability in translation makes the book special.

Even so, much remains undone, and Lewis-Beck is far more likely to have the first than the last word on a number of his claims. For instance, a number of recent papers on the U.S. case have stressed the importance of collective and prospective judgments; and one is left wondering just how different the United States and Europe are in this respect and why. Nor is much said about the effect on public reactions of governments actively taking responsibility for the economy or of dramatic government actions focusing attention on it, two factors that help explain why the British case is the clearest exemplification of the model. And much remains to be done to deepen the analysis. Lewis-Beck says, "Anger counts." But the existence of an angry response is unexplained. Does emotion dominate reason in certain people? Under certain circumstances? Do the media have a role? Where, generally, does the information come from on which economic evaluations are based? Finally, some of the results jump around a lot in the context of changing controls, especially in the Italian case. Readers will have to decide for themselves (and probably do further work to do so) about the robustness of the findings. Nevertheless, this brief, clear monograph in empirical comparative politics is likely to remain a sort of industry standard for quite some time.

JAMES E. ALT

Harvard University

Policy Making in China: Leaders, Structures, and Processes. By Kenneth Lieberthal and Michel Oksenberg. Princeton: Princeton University Press, 1988. 456p. $39.95.

Environmental Policy in China. By Lester Ross. Bloomington: Indiana University Press, 1988. 240p. $35.00.

The study of Chinese politics has entered a new era. Scholars now can interview Chinese policy makers and read diverse documentary accounts of policy debates. Happily, new information has revived the intellectual ambition of China scholars. Some are striving to develop broad conceptualizations of the Chinese political system that reach beyond the area specialists to address a larger comparative politics audience.

Kenneth Lieberthal and Michel Oksenberg are in the forefront of this effort. Their *Policy Making in China* is the first comprehensive analysis of the Chinese political system since the 1960s classics by Doak Barnett and Franz Schurmann. Exploiting their exceptional access to high-level economic officials (Oksenberg had been responsible for East Asia in the National Security Council under the Carter administration), the authors draw a remarkably full, detailed picture of the Chinese political landscape.

The most significant insight of *Policy Making in China* is that formal institutional structure shapes policy processes and outcomes even in a communist authoritarian system. The structure is bureaucratic; authority is fragmented among the various central ministries and provinces; fragmentation is overcome by bargaining; decisions are made by consensus; and the policy process is protracted and incremental. We learn, for example, that provinces and ministries have equal bureaucratic rank and that conflicts between them can be resolved only at the very top levels of the government (the commissions and the State Council), a fact of crucial importance for understanding the bargaining process.

The interests of social groups are articulated not through autonomous interest groups but through bureaucratic organizations (a finding identical to Jerry Hough's discovery of "institutional pluralism" in the Soviet Union). The Ministry of Coal is expected to advocate the concerns of people in the coal industry. The governor of Guangdong is supposed to press the particular interests of the people in his province. Lieberthal and Oksenberg's accounts of policy deliberations present strong evidence that the Chinese system is structured to encourage the bureaucratic expression of social and economic interests.

While Lieberthal and Oksenberg's book is a landmark contribution to our understanding of Leninist systems, it leaves some serious gaps. Several of these gaps could be filled by bringing the insights of the new institutionalism in political science to bear on the study of China. For example, the authors declare their intention to link elite power struggles with bureaucratic politics, but they never clearly specify the power concerns of the top leaders. What political incentives shape the behavior of top national leaders? (In other words, what is the equivalent of the "electoral connection" for communist elites?)

Again, at the top level, the authors minimize the importance of the institutional division between the Communist party and the government. They argue that because economic policies are made mainly by the government and not by the Party, because top leaders are members of both hierarchies, because both are housed in the same Zhongnanhai compound in Beijing (although the government offices are in the northern section and the Party ones in the southern section), because Party leading groups exist in all government agencies, and because there is a single command structure, Party and government are not institutionally separable. But aren't these facts consistent with a view of the Party-government relationship in which the Party delegates some discretion to government bureaucrats while exercising tight oversight over them?

The framework of delegation relations would also help illuminate the relationship between center and localities. Lieberthal and Oksenberg interpret the constant bargaining between central and provincial governments as a reflection of a "balance of power." Yet they recognize that the center has constitutional authority and the actual authority to appoint local leaders. They offer no clear explanation to reconcile centralized authority with the bargaining power of local governments (a pattern that characterized traditional and republican China as well as communist China). Isn't this an agency relationship in which the center has delegated authority and material resources to local governments to improve overall economic performance and remove certain political risks from the center?

Despite these analytical gaps, scholars hungry for good material for the comparative study of political institutions will relish Lieberthal and Oksenberg's book. Half of its four hundred pages are filled with delicious detail from three case studies of energy policy making in the petroleum, hydropower, and coal industries (the project began as contract research done for the U.S. Department of Commerce to help U.S. firms become better equipped to do business in the energy sector in China). Sometimes the detail becomes excessive and distracts from the authors' powerful general analysis of the Chinese system, giving the book the awkwardness of a hybrid intellectual enterprise. But most of the time, the specificity of the case studies provides strong evidence for the authors' generalizations and prevents their analysis from becoming overly abstract.

While not as comprehensive or ambitious in scope as *Policy Making in China*, Lester Ross's *Environmental Policy in China* is another excellent example of the new China scholarship, based on extensive access to Chinese interviews and documents and with a theoretical agenda that transcends area studies. In fact, Ross's book is more theoretically explicit than Lieberthal and Oksenberg's, introducing analytical models from the economics and public choice literature on public goods to the study of environmental issues in China. Ross's analysis is nonquantitative, and he includes a chapter leading beginners by the hand through theories of collective action.

The focus of *Environmental Policy in China* is policy implementation, not policy making.

Ross is interested in assessing the environmental and economic consequences of three different modes of natural resource management: bureaucratic-authoritative (government planning), campaign-exhortation (mass campaigns), and market-exchange (private property). In each of four resource issue areas (forests, water, natural hazards, and air pollution), he discovers a different mix of these modes, enabling him to evaluate the modes' relative success at protecting natural resources.

The book concludes that market-exchange is most effective at protecting the environment, an argument strongly supported by Ross's extensive documentation of the short-sighted abuse of the environment perpetrated during Mao's reign (by both planners and ideologues). Ross rightly notes that environmental protection has attained greater policy priority since the advent of post-1978 market reforms, but is he correct to infer that marketization itself is the cause? Decollectivization of agriculture may explain some increase in afforestation, but it also produced substantial neglect of collective irrigation projects. (Ross claims that the negative effects of marketization are due to China's inadequate legal framework, but then is "market-exchange" as yet only an ideal type in China?)

Both *Policy Making in China* and *Environmental Policy in China* are first-rate examples of the new China scholarship that shed light on the Chinese system and return it to the intellectual domain of comparative politics. *Policy Making in China* begins to open up and describe the contours of a decision-making process that formerly was a black box; and *Environmental Policy in China* utilizes public goods models to give us more rigorous, objective standards for policy evaluation.

SUSAN L. SHIRK

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Aging and Political Leadership. Edited by Angus McIntyre. Albany: State University of New York Press, 1988. 332p. $54.50 cloth, $17.95 paper.

Aging and Political Leadership is a catchy title, offering the hope for insight and learning within a critical theoretical terrain, still uncharted and largely unexplored by political science. This book is a collection of 12 essays—eight previously published—that provide case studies of aging effects in the careers of politicians, most of whom achieved positions of top leadership. The editor also contributed an introduction and conclusion that are quite helpful. The cases are diverse geographically, treating leaders in Germany (Brandt), England (Sir Oswald Mosely), the Soviet Union (Stalin), Israel (Begin), Australia (Henry Parkes), China (Mao), Peru (Haya de la Torre) and the United States (Reagan) and longitudinally, spread out over a period of 150 years. The cases also address different points within careers, one set dealing with early adulthood and middle age and focused on adaptations in midlife crises, and one set dealing with old age and focused on adaptations to the inevitability of loss of power and death. Finally, the book provides two essays that deal with Ronald Reagan. The blend of case studies is intellectually provocative, encouraging comparative thinking within cross-cultural and developmental frameworks. In this sense, the organization of the book works and I found it stimulating and frequently fun to read. What I took away from the reading—what I learned from these essays—is another matter.

Reading through this book as a behavioral scientist, I was alternatively exasperated and intrigued by the self-consciously qualitative, psychoanalytic tone of its material. Frankly, I do not know what to make of the statement, "Mao's practice of first supporting the Red Guards in their struggle against aging party functionaries and then condemning and punishing them . . . had its source in his ambivalent feelings toward his father" (p. 293). Maybe yes, maybe no, but how could we ever possibly be sure? A concluding passage in an essay discussing Reagan's "L-shaped Mind" provides the disclaimer, "This representation of Ronald Reagan is very much my representation, a function of my identity, as your representation of Ronald Reagan would be a function of yours and his of his" (p. 257). No doubt, but again, what is the reader to make of it? As a behavioral scientist who is not trained in psychoanalytic theory, I evaluate these essays as sources of provocative ideas and interesting hypotheses that might stimulate rigorous, systematic research into aging and political leadership. In this sense, reading this book will reward those interested in leadership, comparative politics, political psychology and elite behavior, and political gerontology, as well as those specializing in psychobiography.

Two general problems of broad theoretical concern emerge in these essays. One has to do with the interaction between political frustration and personal ambition within careers. This interaction presents a political problem when frustration and failure in achieving personal goals (lost elections, offices, etc.) coincide with midlife identity crises. Although a personal dilemma for many people in middle age, it presents a potential political problem when power holders undertake personal adaptations within the public arena—for instance, Sir Oswald Mosely's embrace of fascism and anti-Semitism in the early 1930s.

A second and most substantial problem involves how leaders adapt to impending enfeeblement (biological and political) and the prospect of death, especially where tenure in office is not constitutionally constrained. McIntyre quotes Djilas, Tito's biographer, as observing that "political death is the most horrible death of all" (p. 285). Generalizing across the case studies, McIntyre distinguishes four models of leaders' adaptation to old age: *manic, mournfull, narcissistic,* and *oedipal.* The manic ("reliance on the ego-defense mechanism of denial") and narcissistic (either positively transforming "self-love into love of the process" or negative identification "with the idealized superego") models appear commonly and in conjunction in the cases examined. In its negative form, the narcissistic model leads to "rigidity, compulsiveness, intolerance and even involutional depression" that sometimes results in fanaticism. In the manic model, leaders hold tenaciously to power, pursue immortality and, in some cases, rejuvenation (e.g., by reinvigorated ideological commitment, by forging alliances with the young, through physical and cosmetic renewal, and cult of personality). Whether only of heuristic value or not, McIntyre's models highlight the problems of leadership transition where aged leaders hold power. Recalling Ronald Reagan's yearning for a third term despite the obvious signs of his aging, it may be clear that aged leaders tend to have great difficulty in releasing the reins of power and that this presents a public, as well as a personal, crisis, with potentially quite destructive social and political consequences in some countries.

In a world with so many aged power holders, the problem of aging and political leadership is of paramount significance. This book does not (as I had hoped it would) bring history and research in the field of cognitive psychological gerontology to bear on political leadership. The psychobiology of aging from middle into old age involves complex changes in emotionality and cognitive function that must critically affect qualities of leadership by the aged. Political theory and practice could well benefit from an active program of research into the interaction of aging and leadership processes. McIntyre's book provides a useful step in that direction by stimulating thinking about that interaction.

JAMES N. SCHUBERT

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Revolution and Reaction: Bolivia, 1964-1985. By James M. Malloy and Eduardo Gamarra. New Brunswick, NJ: Transaction Books, 1988. 256p. $29.95.

In November 1965 Robert Kennedy launched on a momentous Latin American tour that brought him face to face with the human dimension of revolution. Instead of the white tie receptions, bullfights, and model U.S. businesses scheduled by the typical U.S. embassy tour, Kennedy insisted on seeing the real Latin America: slums, mines, peasant farmers, and radical Marxist students. An encounter with Marxist miners in a dank and dusty Chilean coal mine prompted this blunt Kennedy reaction: "If I worked in this mine, I'd be a Communist too" (Arthur M. Schlesinger, Jr., *Robert Kennedy and His Times* [1978], p. 696). Hardly a week in Latin America and Robert

Kennedy had recognized a stark, endemic reality—class oppression.

Revolution and Reaction, by two established Bolivianists, James M. Malloy and Eduardo Gamarra, is fundamentally a solid and most welcome addition to Bolivian studies. Yet the authors have chosen to ignore the central role of class struggle in Bolivian politics, economy, and society with the disclaimer that "Bolivian politics simply cannot be explained in purely class terms" (p. 73). Indeed the work appears to be two books. The first is a most interesting, insightful, and meticulous description and analysis of the ins and outs of Bolivia's "political class" in the 20 years since the 1964 fall from grace of the National Revolutionary Movement (MNR), the revolutionary party that spearheaded the 1952 revolution. The second, replete with jargon and pseudotheory, seeks to explain Bolivia's "political economy" as the postrevolutionary struggle over a "populist state-capitalist" model of development, an "antipopulist state capitalism," and the more recent "neoliberal capitalism."

The tone of *Revolution and Reaction* is one of understandable frustration with Bolivia's "regression" and its recent failures to develop its economy and lessen dependency, to square the circle of populist demands with resource scarcity and fiscal austerity (democracy versus debt), to establish governing structures that can provide long-term legitimacy and stability, to contain military intervention, or to modernize politics from "more primitive modes of personalistic rule" into a "formalized legal framework" (p. 38). Much of what perplexes Malloy and Gamarra can be summarized by formulating into questions two classic pronouncements, the last attributed to Simón Bolívar himself: Why is Bolivia a beggar upon a throne of gold? and Why is Bolivia inherently ungovernable? The answers—class exploitation and rule by a corrupt few—may be so simple that few self-respecting scholars would dare to starkly propose them, and *Revolution and Reaction* is no exception. The authors' care to avoid "facile" explanations may distract them—and unfortunately the reader as well—from a real understanding of the Bolivian condition.

If class analysis rings true in any country, it does in Bolivia. Indeed, the very arguments used by Malloy and Gamarra to weaken the class thesis only further underline its relevance: the class polarization between the central labor union and Bolivia's private entrepreneurs representing most local capitalists is described as "in effect a class struggle"; and the "ideological conflict over models of the political economy" (capitalism versus socialism) is called a "kind of quasi class struggle" (pp. 73, 169-70); and the rise of a cohesive and powerful private sector is hailed as a new national bourgeois class. In short, although intraclass divisions exist in Bolivia, these have not negated the relevance and, in my mind, the primacy of class struggle.

Bolivia's government is controlled by a small, often corrupt, ruling class, both civilian and military—not hegemonic or monolithic by any means—that pursues capitalist modes of development, either state capitalism or the "neoliberal," private sector variety. Malloy and Gamarra portray the "state" as a predatory entity that confronts "civil society" (pp. 37, 108, 139-41, 148, 187). Why "civil society"? What other society is there? Who or what, then, is the state or the society? It seems that the state is the military and the political class that at the time controls the machinery of government and state economic enterprises, and civil society is the unarmed, often marginalized, civilian interest groups and classes, that is, the people.

By the conclusion of the book, the authors have revealed that the majority of Bolivians (workers, peasants, and Indians) have no more effective say in the running of their government or economy today than before the 1952 revolution and that the "key institutions of the central state" are weak and incohesive because they fail to command popular legitimacy (p. 139). Bolivia, therefore, is "ruled" (not governed) most effectively by naked authoritarian power. Given this brutal and undemocratic backdrop, what does it mean for *Revolution and Reaction* to bemoan the fact that Bolivians are so ungovernable? The central dilemma boils down to this: Bolivian governments are powerful enough to repress citizens and loot the nation, but too weak really to govern!

Another problematical dimension is the Alice-in-Wonderland-like use of words. Despite the book's title, *Revolution and Reaction,* the authors reject the interpretation that any of the post-1964 governments (perhaps with the exception of Garcia Meza, 1980–81) were counterrevolutionary. Neither Barrientos (1964–69) nor Banzer's regime (1971–78), "although harsh and clearly anti-Left" was "in any real sense counterrevolutionary" but was part of an "ongoing process of trying to impose on the forces unleashed by the revolution a new societal model of political economy" (p. 72). In effect, there can be no reaction because everything is integral to the ongoing revolutionary process. Similarly Paz Estensorro's 1985 accession to rule was "not so much an end to the revolution as a readjustment of the relationship between the state and the market in the state capitalist system" (p. 228). Oh well, in the spirit of Alice's Mad Hatter, "Words mean whatever I say they mean!"

WALTRAUD QUESTER MORALES

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Politics of Scandal: Power and Process in Liberal Democracies. Edited by Andrei S. Markovits and Mark Silverstein. New York: Holmes & Meier, 1988. 300p. $49.50 cloth, $19.95 paper.

Why is this excellent collection of essays on topics as scintillating as Watergate, Greenpeace, Profumo, the Lockheed scandals, and the Lavon affair so unsatisfying?

Certainly, it is not in the quality of the individual essays themselves. The volume avoids the characteristic problem of many collections of original essays—a few high quality, provocative analyses interspersed among a larger number of workmanlike, argumentative, or thinly supported ones. On the contrary, these studies of political scandals in the United States, Canada, Britain, France, Italy, Germany, Austria, Japan, and Israel are of uniformly high quality.

Nor does the central theme and treatment of the material vary significantly from one essay to the next. Instead, each succinctly describes the scandal under analysis, identifies the major political, cultural, and institutional arrangements of the country pertinent to what the editors refer to as "scandology," explains how these arrangements gave rise to the scandal in the first place and how these factors also affected the political response to scandal, and then draws some conclusions about the impact of the scandals on the politics of the country in which they occurred.

Each reader will have his or her own favorite account. I especially enjoyed the French and Italian analyses: the French because Stephen Bornstein sets forth so clearly and persuasively the factors in French politics that helped leading politicians avoid disgrace after having covered up their role in the illegal sinking of a ship and killing of a crew member in foreign waters; the Italian, because Judith Chubb and Maurizio Vannicelli explain how the party system of Italy creates a context in which scandal is so commonplace that it is hardly scandalous.

But it is really quite unfair to select these two essays out for special praise; others in the collection identify with equal perspicacity how the salient features of the political system under study affect the politics of scandal. In fact, the context in which the scandals are placed are described so thoroughly that Theodore Lowi expresses my view exactly in his foreward to the book: "If I were qualified to teach comparative politics, which I am not, this would be my text" (p. xii). As an introduction to the key political institutions of these countries that "catches each country in the act of being itself" (p. xii), this collection can hardly be beat. And as long as the students have the sharp eyes of the young, they will not be put off by the publisher's decision to cut costs by cramming nearly a thousand words on the two sides of each six-by-nine-inch leaf.

No, the dissatisfaction is not with the elements but with the whole. As much as we learn about the politics of these countries, we learn very little of a general nature about the politics of scandal. Instead, the studies focus on British and French exceptionalism, Italian distinctiveness, Japanese pecularities, and the like. Indeed, they confirm the old saw that the difference between the fields of international politics and comparative politics is that the former studies two countries instead of one.

The editors' main effort at cross-national comparison is the suggestion that scandals are limited to liberal democracies. The point is not documented, however, since nondemocratic systems are not included in the study. There is also an implication that Anglo-American regimes are less protective of the perpetrators of scandal than are other regimes, but the cases do not unequivocally support this proposition either.

The reasons the volume makes but a limited contribution to cross-national comparisons are not easy to ascertain. Two possibilities come to mind: first, that the editors and the authors did not work assiduously enough at developing concepts and indicators that would facilitate cross-national comparisons (given the consistency of how the material is handled across cases and the uniformly high quality of the essays, I am reluctant to hasten to this conclusion); second, that Oakeshott was correct in arguing that cross-national comparisons are inherently difficult, if not impossible, to make, so that the best use of comparative analysis is to understand more profoundly the uniqueness of each country. Although little in this volume refutes Oakeshott's contention, it, too, is ultimately unsatisfying.

PAUL E. PETERSON

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Political Parties: Organization and Power. By Angelo Panebianco. Translated by Marc Silver. New York: Cambridge University Press, 1988. 360p. $54.50 cloth, $16.95 paper.

This is an ambitious and remarkable book, in the tradition of Roberto Michels and Otto Kirschheimer. Panebianco is interested in political parties as organizations, not as units of political party systems. As such, the work is appropriately read side by side with Giovanni Sartori's already classic study, *Parties and Party Systems* (Cambridge, 1976). Like Sartori, Panebianco combines theoretical formulations and a wide-ranging search for empirical evidence that permits some evaluation of the propositions he poses.

Panebianco's basic argument is deceptively simple. Partly from Michels, he derives the assumption that leaders of political parties are "entrepreneurs" who go in search of power and then try to maintain it intact. Their commitment to homeostasis is self-conscious as well as self-serving, and their efforts are to safeguard the *stability* of the party organization (i.e., its internal authority channels), not just the party for the party's sake.

Panebianco's contribution is to show that the success of such leadership behavior is inevitably conditioned by a number of factors, including the nature of the party's origins and subsequent history, the degree of its institutionalization, and also the party's external environment and the kinds of impulses and challenges that derive from it. Similarly, these factors will condition how fast, as well as how, party organizations are transformed. Levels of institutionalization, in particular, are expected to determine the party's autonomy (e.g., from internal groups like trade unions or from geographically based factions), as well as its propensity to make organizational changes in response to external pressures.

The empirical evidence is drawn from a half-dozen "oppositional" parties (including the Communist parties of Italy and France, the German and Italian Socialist parties, the French Socialist Party, and the British Labour party) and three "governmental" parties (including the Christian democratic parties of West Germany and Italy, and Britain's Conservative Union). There are problems here—of reliance on limited secondary sources and of superficiality of historical and analytical treatment—that the author himself disarmingly acknowledges. Even so, the basic points he wants to establish as to the conditions that bring about strong or weak institutionalization and greater or lesser dependency of the party on its external environment are persuasively made. Thus, the work represents a considerable improvement over the earlier and now somewhat outmoded formulations of Maurice Duverger.

It is in the evolution of the mass-bureaucratic party into what the author calls electoral-professional parties that Panebianco is the most thought-provoking and, perhaps, controversial. Well-known modifications in the environment, such as changes in the stratification system and the revolutions in communications systems, are said to represent powerful forces that act on all political parties. Their responses are conditioned by their internal organizational characteristics and by the degree of fragmentation that the party system itself manifests. One way or another, though, and sooner or later, the parties will change. In this sense, Panebianco is fully in agreement with Kirchheimer's formulation of a generation ago.

Environmental transformations mean that parties no longer create collective identities on the basis of which they both integrate segments of the electorate and guide its "expressive" behavior. Similarly, parties not only come to lose even more of their exclusivity (always exaggerated) in the selection of candidates for public office but also their critical role in the formation of public policies. As the electoral-professional party emerges, the party itself is marginalized, and greater space is accorded interest groups, state bureaucrats, the mass media, promoters of single-issue politics, and other such segments of the population.

This is obviously the stuff of so-called corporatist modes of democratic politics that have been egregiously oversold to an inattentive scholarly community. Panebianco is careful not to fall into this discredited conceptual and empirical morass. He suggests, instead, that the politics of postindustrial society, having relegated all sorts of historical and ideological cleavages to history's dustbins, may very well be based in the future on shifting, ad hoc and antiestablishmentarian coalitions arrayed against official power holders.

Far from welcoming such trends, Panebianco correctly identifies aspects of them that should be disquieting for democratic societies and for those who theorize about them. Political parties that become, in his words, "only convenient tags for independent political entrepreneurs" (p. 274) are little different from ad agencies and polling organizations that together and for a fee will package and market electoral candidates. Not too far off the horizon, no doubt, are the specialists in the leveraged buy-outs of political parties and, perhaps, of governments themselves.

If any of this sounds plausible, it is doubly disappointing that Panebianco excluded U.S. political parties from his analysis on grounds of their "exceptionalism." Exceptionalism may be true of the historical origins of these latter organizations. But if, as Panebianco himself seems to believe, the U.S. parties are the prototypes of what the future holds for their counterparts in Europe, a full understanding of the processes of party development and transformation and a test of Panebianco's theories concerning both would depend on an eventual correction of this omission.

The book, by the way, is beautifully translated.

JOSEPH LAPALOMBARA

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Losing from the Inside: The Cost of Conflict in the British Social Democratic Party. By Patricia Lee Sykes. New Brunswick, NJ: Transaction Books, 1988. 320p. $32.95.

Several books on the British Social Democratic party (SDP) have been written prior to this one. The previous ones have tended to be either journalistic or participant observer accounts. This one differs in being a scholarly analysis, both a strength and a weakness.

The author confesses that part way through her research she suddenly realized that the SDP, contrary to its rhetoric, was not a new type of political party but the same sort of organization as other British parties. The main aim of the book is to use the experience of the SDP from 1981 to 1987 to demonstrate the inadequacies of the Downsian theory of party competition. Crude Downsianism, or the "literary theory" (as the author labels it), is rather a straw man that the author has little difficulty in scoring against. Yet the challenge to Downsianism gives the book its analytical framework and raises it a cut above previous works on the party.

The book is based on the author's dissertation, but includes a good bit of additional research. Too many footnotes are a major distraction. Sentence after sentence and even more than one clause within a sentence carry citations. But at least such scholarly concern has produced a study that is quite thoroughly researched. In particular, the author has done an excellent job in interviewing the key figures (in some instances more than once). She has gained valuable information and knows how to use a quote tellingly. Consider the following revelation of the monumental egos among SDP activists: David Owen said, "From the beginning, I knew it would have been better without involving Roy [Jenkins], but Bill [Rodgers] and Shirley [Williams] were vacillating. . . . It was truly a Gang of Three, and we should have kept it that way" (p. 111) and, "I was against every decision taken during the first two years" (p. 123). Roy Jenkins said, "The others could never have done it without me. . . . I was the founder" (p. 112).

The author claims that students of British parties instructed by the Downsian perspective have expected unified teams and have ignored the impact of intraparty divisions. The SDP certainly provides evidence of a wealth of internecine struggles. Owen's concept of leadership differed significantly from that of his colleagues (thus guaranteeing conflict) and explains how personal power struggles and vanity made a shambles of both the 1983 and the 1987 Alliance election campaigns.

Sykes's chapter on the media is especially interesting. She offers a threefold typology of reporters' roles and shows how journalists of each type, simply by playing their chosen role, helped to damage the Alliance by perpetuating the image of dissension and division. Her analysis also explains, however, how attempts by Alliance leaders to manipulate the media for personal advantage helped to perpetuate this negative perception.

The message of the book seems rather ambiguous. Most of the time the author seems to believe that the behavior of the SDP leaders was all a matter of personal power struggle. The party was done in by cynical Machiavellian maneuvering as each leader sought his or her best interest even at the expense of the party's good. Yet in her final paragraph she refers to "the lesson the SDP teaches, that conflict is structural not personal" (p. 202). Insofar as the two can be separated, this appears to mean little more than that minor parties have a hard time under the British electoral system and the Westminster parliamentary system. She argues that conflict among the Gang of Four was a matter of image, not ideology, and that clashes between David Steel and David Owen were a matter of style, not substance. Yet in reflecting on developments after the 1987 election, she seems to back off from her main theme to comment, "It is possible that a pure power struggle evolved into an ideological clash, as Owen developed the substance to go with his style" (p. 200).

This book, like its less scholarly predecessors, is more interesting for the information it provides than for the theoretical insights it offers. (Concluding that "the party that wins on the center-left will be the party that discovers coherent and sensible solutions to the problems of a declining economy and a lost empire, and then, that comes closest to speaking with a single voice" (p. 202) can hardly have required research). Nonetheless, the fact that the book was written by a scholar seeking not just to describe but to analyze, is an improvement over the competition.

JORGEN RASMUSSEM

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Policing Liberal Society. By Steve Uglow. New York: Oxford University Press, 1988. 176p. $29.95.

The Police, Public Order, and the State: Policing in Great Britain, Northern Ireland, the Irish Republic, the USA, Israel, South Africa, and China. By John Brewer, Adrian Guelke, Ian Hume, Edward Moxon-Browne, and Rick Wilford. New York: St. Martin's, 1988. 241p. $49.95.

Undercover: Police Surveillance in America. By Gary T. Marx. Berkeley: University of California Press, 1988. 280p. $25.00.

While the three books under review deal with issues relating to policing, they show differences in emphasis and conceptualization, differences influenced to a great extent by the national concerns and experiences that surfaced in Great Britain and the United States during the 1970s and 1980s. Thus, given the recent debate in the United States over abuses of power by local police Red Squads and FBI political surveillance, Gary Marx focuses on covert activities of the police. In contrast, responding to the debate over the police role in the divisive strikes, antinuclear demonstrations, and religious-ethnic conflicts in Great Britain and Ireland in the 1970s, the authors of the other two studies under review focus on overt crowd control and law-and-order activities.

Steve Uglow, a professor of law at the University of Kent, and the five authors of *The Police, Public Order, and the State* (who, with the exception of Wales University social scientist Ian Hume, are political scientists and a political sociologist at Queen's University, Belfast) are interested in police issues less as criminologists or students of public administration than because of the tensions and conflicts dominating recent British politics. Uglow examines the role of police in a constitutional system and specifically explores the limits and legitimacy of that role. One of his principal concerns centers on the increased militarization of the police and what he perceives as a shift from a service (servant of the community) to a force (imposer of order) orientation and the attendant pressures on the police to maintain order at a time of major economic and political divisions. In his essentially derivative survey, Uglow laments the increasing autonomy accorded the police and argues for making the police more accountable for their decisions and exercise of power and for strengthening safeguards "to build a constitutional corral for the office of constable" (p. 148) while at the same time countering "the ubiquity of the simplistic 'law 'n' order' approach" (p. 148). More a political history of policing in modern Britain, Uglow's thin monograph offers little new information or insights into the role and function of police. His study is essentially more anecdotal in approach than rigorously researched and analyzed.

While the authors of *The Police, Public Order, and the State* adopt a comparative approach, they share Uglow's perspectives of the police as agents of the state and his concern over the militarization of the police function. If the comparative approach offers the prospect of a broader perspective—the authors devote separate chapters to survey the role of the police in Britain, Northern Ireland, the Republic of Ireland, the United States, Israel, South Africa, and the People's Republic of China—their narrative conveys a picture of an underlying uniformity in the police function. Because the authors survey the history of the police in these radically different political and economic systems in a mere 208 pages, the value of this comparative analysis is decidedly limited. The resulting narrative is most perceptive in the chapters on the police in Britain and the two Irelands, of which the authors are most directly familiar, but is decidedly superficial in the chapters on the other states. Surprisingly, given the distinctly different political problems and constitutional systems of Israel, South Africa, the People's Republic of China, and the United States, the authors fail even to discuss the police's political surveillance activities and the related questions of professionalism, accountability, and constitutionality.

A professor of sociology at the Massachusetts Institute of Technology who has written extensively on policing issues, Gary Marx, in contrast, focuses exclusively on police undercover operations and their attendant ethical and oversight issues. Methodologically more rigorous and based on extensive research of sources (including the relevant secondary literature supplemented by interviews with police officials and defense and prosecuting attorneys and research into congressional hearings, court cases, and local and federal police records), Marx's study will command the interest of students of public administration and the police. In an often thoughtful, carefully reasoned, and well-organized survey of the problems and opportunities attendant on undercover operations (and funded by a grant from the Twentieth Century Fund), Marx offers a compelling case for the value of such operations under specified conditions. His concluding paragraph summarizes his overall analysis: "In a democratic society, covert police tactics, along with many of the other surveillance techniques, offer us a queasy ethical and moral paradox. . . . Sometimes undercover tactics will be the least bad. Used with great care, they may be a necessary evil. The challenge is to prevent them from becoming an intolerable one" (p. 233).

This quote captures Marx's balanced, evaluative approach. Drawing on a series of examples, Marx addresses the ethical problems, the policy advantages and consequences, and the controls required to limit undercover operations. Paradoxically, however, he fails to explore the distinctly different problems posed by the use of undercover operations during criminal investigations (such as the FBI's ABSCAM investigation) and their use during intelligence investigations (such as the FBI's investigation of the Committee in Solidarity with the People of El Salvador [CISPES]). Because intelligence investigations do not result in criminal prosecution and are governed by executive guidelines (which can be changed—and indeed were changed by the Reagan Administration in 1983), they pose a series of quite different policy and oversight issues not directly addressed in this study. While the relevations of the FBI's investigation of CISPES broke after Marx had completed the research for this study, the fact of the matter is that Marx had not anticipated this kind of case and the serious questions it raises about controls and the counterbalancing criteria for assessing the value and consequences of undercover operations. Apart from this serious omission, Marx's rigorously analyzed monograph constitutes an important contribution relevant to a current national policy issue.

ATHAN THEOHARIS

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Making Mondragon: The Growth and Dynamics of the Worker Cooperative Complex. By William Foote Whyte and Kathleen King Whyte. Ithaca, NY: Industrial and Labor Relations, 1988. 336p. $36.00 cloth, $16.95 paper.

Forty-six years ago, William Foote Whyte picked a fight in these pages with the U.S. political science profession. With this book the reader can judge who won.

In the August 1943 *American Political Science Review* the author of *Street Corner Society* challenged the profession to "take an interest in politics" and "establish themselves as participant observers" so that practical knowledge of politics and its relations to social structures could be understood. His challenge was met with accusations of rank positivism and indifference to moral issues by John Hallowell in a lead article in the *Review* a year later (a symposium followed in 1946 with contributions by Hallowell, Whyte, Almond, and Dexter).

Making Mondragon is the testament of a husband-and-wife team for whom the provision of practical solutions to social, economic, and political problems has supplied the motive for distinguished contributions to sociology, anthropology, and industrial relations. They first worked together on *Street Corner Society*, and Kathleen King Whyte coauthored the present volume after more than a decade of involvement with Mondragon.

The village of Mondragon, in the Basque country of northern Spain, is at the center of a network of about a hundred industrial and service cooperatives. The network is tied together by such second-order cooperatives as a bank, a social service system, a research institute, and an "entrepreneurial" institute. The Whytes sketch the history, beginning with a school founded in 1943, through the establishment of the first coop in 1956, on to the present day when, under the brand name *Fagor*, the network includes Spain's largest appliance manufacturer. The brief history highlights the critical decisions that propelled the quick expansion in the sixties and the careful response to recession in the eighties. All of the classic issues of cooperative effort are met and dealt with: capital formation, factionalism, oligarchy, adaption to technological change, the humanization of work, the painful processes of economic adjustment, and the question of organizational culture.

The etiology of this book lies deep in the history of social science. Personally, professionally, and politically, it can be taken as a revelation of what social science might have been and could possibly become.

On the personal side, it is one more evidence of that abiding interest of middle-class reformers in the life of the less privileged, a motive that Whyte acknowledges freely in an appendix to *Street Corner Society* and that guided him through more than 40 years of work in the area of industrial relations at Cornell. In Mondragon, the Whytes found something of what they had been looking for all these years: a system of human relations in an industrial context that is workable, humane, and progressive. Here is a "sociotechnical" system (to use Eric Trist's phrase) that integrates technology, organizational structure, and social processes rather than adapting the latter two to the first. Here is a working illustration of participatory management that has overcome all obstacles in more than 30 years, including a severe recession, without losing its dynamism.

Professionally, the work is a singular achievement in what Whyte identified in the July 1987 *Industrial and Labor Relations Review* as the "alternate stream" of organizational behavior research. As opposed to the "mainstream," where research and practice are separated and research is constrained by rigorous specification and measurement of variables, this stream emphasizes applied research and involvement in organizational change. For example, the "study action" teams formed at the Xerox Corporation with assistance from Cornell University brought union members and management together to research, and act on, cost-cutting measures that saved jobs. In Mondragon, a larger-scale participative research project is in place through the work of Davydd Greenwood, a Whyte associate. Together they have built in to the Mondragon "experiment" an ongoing participative research function for which the Whytes' book is prologue—see *American Behavioral Scientist* 32: 513–51. Whyte feels the mainstream is "running dry"; and his own examples of the usefulness of the "alternate stream" are listed in the August 1986 *American Sociological Review*.

Whyte's human relations approach to participative management is the path not taken in U.S. economic development, or taken only belatedly. The Japanese got there first (in part due to the influence of Whyte on a Japanese student who founded the Japan Human Relations Association in the early 1950s). Whyte thinks the United States was so slow in part because of the mainstream where the very obscurity of findings played in to the dismissal of social science generally as a guide to policy making.

That Whyte should crown his career by adding a third association presidency to his distinctions, that of the American Sociological Association in 1981, suggests there are many who agree with him. In his presidential address, reprinted in the February 1982 *American Sociological Review*, Mondragon served as his key example of what should be studied in a reoriented sociology committed to "the discovery, description, and analysis of *social inventions for solving human problems*" (p. 1).

Politically, producer cooperatives are the stepchild of democratic socialism, spurned by Beatrice and Sidney Webb, shunned by most unions, and dismissed by Marxists. With the decline of the British Labour party, the weakening of unions generally, and the dismantling of Marxist-Leninist regimes, producer cooperatives have become more interesting to the Left. At the same time, those on the Right who seek social legitimacy for a market-driven society find cooperatives attractive. Mondragon is being studied with care by scholars from all over the world, including the Soviet Union. This book will be at the top of the reading list—not as a manifesto but as a pragmatic and thoughtful exploration of the critical issues.

The one shortcoming of the book—a consideration of the relation of these findings to the comparative work done by Keith Bradley and Saul Estrin of the London School of Economics and to the U.S. debate over cooperatives found principally in the work of David Ellerman and Edward Greenberg—can be remedied by other scholars. The supreme test of Mondragon will come in 1992 when it has to confront a European free market. However, as this writer observed in a visit last fall, preparations are well under way to meet major multinational competition.

Altogether, the book is an exemplar of a bold, inventive, and altogether practical social science that deserves serious attention by scholars across the fields and disciplines. *Making Mondragon* is normatively inspiring and politically fascinating.

KENNETH R. HARRIS

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The Transition to Democracy in Spain and Portugal. By Howard J. Wiarda. Washington: American Enterprise Institute for Policy Research, 1989. 430p. $28.50 cloth, $17.50 paper.

Most of this book consists of essays published by the author during the period 1976–87. Five present a restatement of Wiarda's now-familiar argument that Iberian political culture and social relations are, and have been, characterized by elements of corporatism and organic statism. While the empirical evidence in support of this claim is not entirely persuasive, Wiarda's discussions of the origins of corporatist thought and the evolution of political and social relations in Spain and Portugal are interesting. Chapter 8 ("From Corporatism to Neo-Syndicalism") is particularly well written and raises questions concerning the extent to which South European patterns of political development are distinct from those of the rest of Europe. Two of the other essays deal with foreign policy issues linking Spain and Portugal with Latin America and the European Community, another (first published in 1975) discusses "change and continuity in the Portuguese armed forces movement," one is a rather dated discussion of political parties in Spain and Portugal, and one presents an overview of Portuguese elections.

The central theme of this book will attract the most attention, and deserves the most careful scrutiny. Briefly stated, the author contends that "the realities are—however uncomfortable these facts may leave us—that the democratic transitions in both these countries are still incomplete, democracy has not been fully consolidated and may not even now reach deeply into the society or its consciousness, and there are some powerful currents, attitudes, and institutions in Iberia that are not at all convinced that democracy is the best or only form of government" (pp. 1-2). Wiarda dismisses the huge and growing literatures on Iberian democratization (in English, Portuguese, Spanish, and Catalan, very little of which he acknowledges or cites in an accurate or appropriate manner) by asserting, "The literature we have tends to be celebratory rather than analytical. But celebration cannot be used as a substitute for hard analysis, nor can wishful sociology and wishful political science be used to gloss over facts with which we may be uncomfortable" (p. ix). If, as stated on the dust jacket, the intention of the author was to write a book that would be "controversial and certain to stir discussion," it is clear that he has been remarkably successful on at least the first count.

Wiarda does raise some interesting and critical questions. He expresses concern, for example, over the low level of mass involvement with political and secondary organizations and over the extent to which parties in these countries may be inadequately institutionalized. These are, indeed, significant features of Spanish politics that deserve further study.

But in an effort to challenge the conventional wisdom concerning the success of democratic consolidation in Iberia, he also makes several bold statements that are factually incorrect. He states, for example, that both Spain and Portugal are "very conservative," even though survey data indicate that the political center of gravity in both countries is left of center, and more progressive than in the rest of Europe. In reasserting traditional clichés about Iberian cultures, he ignores the profound processes of secularization, value change, and alterations in social relations that have taken place over the past two decades.

To cite another example, he contends that Portuguese and Spanish voters have become so alienated from electoral politics that "it is rather like the voting behavior of French peasants in earlier times who smeared excrement on the ballots or voted communist or fascist as a protest against the series of ineffective, Fourth-Republic governments in Paris in the 1940s and 1950s" (p. 7). In fact, Spanish electoral data reveal that there has been no significant increase in blank or spoiled ballots since the first election (they have consistently totaled about 2% of all votes cast), while in Portugal they declined rather dramatically from 6.9% in 1975 to about 1% in the 1980s. Support for left- and right-wing extremist parties is also low, totaling about 3% of Portuguese voters and declining precipitously among Spanish voters (outside of Euskadi) from nearly 5% in 1979 to about 1% in 1986. The electoral setbacks experienced by the Communist parties of both countries further undercut the credibility of Wiarda's thesis.

One common feature of Wiarda's treatment of most of the political and social issues he raises regarding the contemporary era in Spain and Portugal is that careful documentation of empirical support for his often iconoclastic assertions is lacking. This seems particularly inexcusable in the case of Spain, which has one of the most rigorously analyzed electorates in the industrialized world. The Center for Sociological Studies alone has undertaken approximately two thousand nationwide surveys (each with an N of over a thousand) during the 1970s and 1980s. Its data are available to scholarly researchers upon request and are regularly published in its quarterly review. By ignoring the massive volume of primary data and secondary analyses of Spain's democratic transition, Wiarda must fall back on a rather journalistic approach to these issues, which is provocative but not persuasive.

A survey of these data is not generally supportive of Wiarda's thesis, at least in the very broad manner in which it is stated. Survey data and behavioral evidence clearly reveal massive popular support for democracy and its key institutions in Spain, as well as the absence of significant support (outside of Euskadi) for antisystem parties or movements. Indeed, the stability of this new democracy under extremely difficult circumstances (such as a high level of unemployment that at one point exceeded 22%) and during a profound restructuring of the state itself (a process that Wiarda completely ignores, asserting, instead, that both Iberian countries have maintained a strongly "centralized bureaucratic state") would suggest that Spain's democratic system, at least, is rather substantially consolidated.

Wiarda could have made a more significant contribution if he had limited himself to less sweeping claims, reflecting the real diversity within Iberia. Indeed, the sharp contrast between Euskadi (which has experienced extreme polarization and violence and within which democratic institutions have been subjected to severe challenges but which Wiarda completely ignores) and the rest of Spain, between Spain and Portugal, or between developed and less wealthy regions in both countries appear to present the most fruitful opportunities for future research.

RICHARD GUNTHER

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INTERNATIONAL RELATIONS

Transformation and Continuity in Revolutionary Ethiopia. By Christopher Clapham. Cambridge: Cambridge University Press, 1988. 304p. $42.50.

The Ethiopian Transformation: The Quest for the Post-Imperial State. By John W. Harbeson. Boulder: Westview, 1988. 200p. $29.95.

Revolutionary Ethiopia: From Empire to People's Republic. By Edmond J. Keller. Bloomington: Indiana University Press, 1988. 320p. $35.00.

Fifteen years after it began, the Ethiopian revolution remains controversial. These three books provide preliminary assessments of the revolution, its consequences for the Ethiopian state, and its theoretical significance. The authors, all long-time students of Ethiopian politics, agree on many of the most salient aspects of the upheaval but differ in their analyses of the subsequent state.

Each book discusses, to varying degrees, the imperial regime, the mobilization phase of the revolution that overthrew Emperor Haile Selassie in 1974, and the consolidation of power and construction of a postimperial state. Harbeson focuses his historical analysis on the forces that held the empire together. The continuing existence of the state during imperial interregnums is evidence that the Ethiopian polity rested on a sense of community deeper than the institutions of the empire. Harbeson keeps one eye on this basis of the state while he traces the political transformation. Over half of Keller's book is devoted to a detailed historical analysis of the ancien régime and the underlying and precipitating causes of the revolution. The author uses Eisenstadt's concept of bureaucratic empire to examine the growth and centralization of power and suggests that Haile Selassie emphasized control over integration. Clapham spends little time analyzing the ancien régime in this volume, but his *Haile-Selassie's Government* (1969) remains one of the seminal books on the topic. This latest book, however, does provide a concise and stimulating treatment of ethnicity and political culture.

The books differ most substantially on the nature of the postrevolutionary state. According to Clapham the state consists of a structure of control, a structure of extraction and distribution, and a group of people who manage the state's affairs. Clapham is clearly interested in institutions and provides the best analysis of the party and the urban and peasant associations to date. Harbeson, following Weber and Rothchild, defines the state as a "set of organizing principles" (p. 14). He explores the state from this broad constitutional perspective and emphasizes the relationship between state and society. According to Harbeson, the military leaders attempted to revolutionize the socioeconomic structures as a foundation for the reconstruction of the Ethiopian state. Insurgents, however, challenged the armed forces' self-proclaimed role as the midwife of the transformation. It is Harbeson's perceptive thesis that the outlines of the postimperial state were hammered out in the course of this military conflict. Keller's approach is more structuralist and questions the extent of state autonomy in relation to both domestic social forces and international influences. The revolutionary transformation, Keller argues, requires the construction of a new "social myth" and much of the postrevolutionary political conflict has been the result of differing perspectives on the appropriate source of legitimacy.

Clapham concludes that the leadership in Addis Ababa has constructed a highly effective, disciplined, and centralized state, while he recognizes that this ability to control has proved antithetical to agricultural development and the resolution of the nationalities question. Keller, in contrast, argues that Ethiopia is a "soft" state that is "largely incapable of predictably following through on its will in an efficient manner" (p. 214). This argument, however, is undercut both by Clapham's and by Keller's own discussion of the very efficient manner in which the regime implemented campaigns for literacy, mass conscription, villagization, and the organization of urban and rural associations. Harbeson agrees that the regime is capable of command-based rule but takes a position different from Clapham by emphasizing the weakness of state-society relations. Harbeson suggests that endemic internal conflict is evidence of the weakening of the state since the revolution. This debate on the nature of the current Ethiopian state will continue because the opposing sides base their arguments on fundamentally different theoretical definitions of the state.

Broad public awareness of Ethiopia's economic policies developed in response to media accounts of the famine and criticisms of villagization and resettlement policies. Each of the authors covers this central topic in depth. Keller blames the soft state's limited resources for the failure of its centrally planned development. Harbeson suggests that the regime's opposition to peasant individualism and its pursuit of an economy controlled from above has led to the poor economic performance. He adds, however, that the problem may be more the result of poor management practices than ideology. Clapham argues that Ethiopia's agricultural policies have entrenched peasants in their existing mode of production under conditions that guaranteed their progressive impoverishment and increasing dependence on the state.

While one is pleased to see these new additions to the study of Ethiopian politics, the timing of their nearly simultaneous publication is somewhat puzzling. Ethiopia proclaimed a new constitution in September 1987 and established a new layer of political institutions. The authors take this transition as their end point. One may argue, as these books do, that this change was more symbolic than real, while still desiring a detailed discussion of the implementation and operation of the current political structures in Ethiopia.

All three books represent important additions to the literature on the Ethiopian revolution and provide new material on theoretical issues relating to revolutionary transformation and the role of the state. For the reader who is unfamiliar with Ethiopian history and politics, Keller and Harbeson include more background material and are good introductions. Clapham's book is narrower in scope but contains the most original and insightful discussion of politics since the revolution. The author's account is chock-full of primary sources and information not published or analyzed elsewhere. Clapham has the ability to move from the details of individual's actions or a single policy step to broad theoretical questions, tying the various threads together into a single argument without missing a step.

TERRENCE LYONS

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Innovation and the Arms Race: How the United States and the Soviet Union Develop New Military Technologies. By Matthew Evangelista. Ithaca: Cornell University Press, 1988. 320p. $32.95 cloth, $10.95 paper.

This book aims to reconcile aspects of the dispute between two schools of thought (the external and the internal determinants) regarding the source of arms races, specifically the U.S.-USSR arms race. Evangelista believes that the common mistake of each school is to maintain that its explanation applies equally well to both superpowers. Evangelista's theme is that internal determinants are more useful for analyzing the development of American military technologies but that the major impetus for Soviet weapons comes from the international arena.

The book is valuable for several reasons. Much of the arms race literature that focuses on the internal dimension uses only the basic elements of organization theory. One of Evangelista's central concerns is to wed what we know about what makes organizations innovative to the historical and political circumstances of the United States and USSR. In discussing the technology development process in the Soviet Union in particular, Evangelista adds measurably to our understanding of what that process is and why it works the way it does. Second, the book is careful not to overstate its conclusions and examines the circumstances under which its arguments are more likely to hold. Third, the author uses a fascinating case study of the development of tactical nuclear weapons by the two countries to test the hypotheses he outlines.

Evangelista also goes well beyond caricaturing the arguments of either the "internalists" or the "externalists" in developing each school's hypotheses. He deals sensitively with the notions of the externalists, for instance, by allowing the international influences to which the United States responds to vary from concern for the cohesion of NATO to worry about the size of the Red Army. And when outlining the application of organization theory to the subject, Evangelista is careful to include change in foreign policy goals and leadership that may affect the utility of the argument. Rather than dismiss or apply only one school of thought for either superpower, he effectively and persuasively analyzes the situations under which different explanations are more likely to obtain.

The book is not without its controversial parts. Evangelista's model of the U.S. development process places much weight on the willingness of the armed services to use and accommodate themselves to new technology. After technocratic initiatives have taken place (stage 1), consensus building (stage 2) and promotion (stage 3) require the services to see the utility of the new technology and support it politically. Much of the research on bureaucratic politics leads to the conclusion that if the new technology fits into what the organization sees as its mission, the process will proceed smoothly but if the technology challenges that mission, there may be opposition. Evangelista recognizes this difficulty and stresses that the services utilize international developments to justify adoption of the new technologies. Thus, for the United States, events (real, imagined, or anticipated) in the international arena are necessary for the utilization of new technologies (stages 4 and 5—open windows and high-level endorsement).

An episode Evangelista presents captures the Soviet process, which can be characterized as operating from the top down, well. In 1949 or 1950 Soviet scientists and engineers began work on an updated jet bomber, the Il-30. Stalin preferred that work be directed toward improving an existing bomber, the Il-28, and ordered, "No distractions. Orient yourself only to the Il-28" (p. 171). Not surprisingly, his orders were carried out. Thus, in the Soviet case international events are necessary to the process in a different way. They serve to motivate high-level response. Technological research is largely controlled by the leadership's priorities. The point is that in both countries international events (or the perception of them) are critical ingredients of the process. Action-reaction models do not capture the Soviet-U.S. dynamic well, but it is too much to say that one country is more responsive to the international environment than the other. They are responsive in different ways.

A second difficulty is dealt with admirably by the author. The case study he uses supports the hypotheses well. But would other cases do so? Evangelista discusses other more recent technological developments and offers strong reasons why some developments fit his model while others do not.

Evangelista's is a major addition to the literature on U.S.-Soviet relations, to the history of the strategic interaction of the two states, and to our understanding of the arms race.

GLENN PALMER

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Development and External Debt in Latin America: Bases for a New Consensus. Edited by Richard E. Feinberg and Ricardo Ffrench-Davis. Notre Dame, Indiana: University of Notre Dame Press, 1988. 272p. $29.95 cloth, $16.95 paper.

Latin American Debt. By Pedro-Pablo Kuczynski. Baltimore: Johns Hopkins University Press, 1988. 272p. $32.50 cloth, $12.95 paper.

As the debt crisis moves from one phase to the next—the latest, like the last, named for a U.S. Treasury secretary—books on the crisis can hardly keep pace with the shifting positions of the key players. Despite the short half-life of any work on this subject, the books under review hold interest for their competing analyses of the Latin American economic predicament and their prescriptions for escaping it. Although the analysts are economists, their diagnoses do not lack political importance. One can detect the intellectual outlines of a debate on Latin American economic policy in the 1990s.

Pedro-Pablo Kuczynski, whose career has encompassed the public and private sectors in both Americas, offers both an authoritative summary of the unfolding of the debt crisis from the 1970s through the Baker Plan and a more questionable analysis for Latin America's economic crisis. Kuczynski's brief is not very different from that of the International Monetary Fund (IMF) or the World Bank. Although he does not spare the commercial banks in apportioning responsibility, his "core problem" in Latin American economic development is the bloated state and the inefficient state enterprises under its control (chap. 3). His assessment, however, is not fully supported by the comparative data presented. The size of the state enterprise sector in Malaysia (table 3) is as large as most Latin American states, yet Malaysia's economic performance in the 1980s was far stronger. As Kuczynski himself admits, the small public sector in Chile did not spare Pinochet's monetarist experiment a deep economic depression. It seems more likely that answers must be sought in the *type* of state intervention that has flourished in Latin America since World War II and the pattern of macroeconomic policy—persistent fiscal crisis and unrealistic exchange rates. Kuczynski only touches upon these alternative explanations and their political roots.

Although Kuczynski's orthodox reading of Latin America's economic plight can be contested, he is far from uncritical of the post-1982 debt strategy led by the United States. He documents the cost of the debt shock to Latin America and points to contradictions in the strategy, particularly changes in the financial markets that made the first-stage strategy of restoring the status quo ante illusory. He criticizes the absence of a coherent plan for restarting growth in Latin America as sharply as those habitually more critical of the IMF and the United States. His own recommendations emphasize increased finance from public sources, particularly the multilateral development banks; continued adjustment in the developing countries; and, as a last resort, deferral of interest payments. These prescriptions are hardly unique to Kuczynski and confront, as he admits, major political obstacles in the industrialized countries.

The essays in *Development and External Debt in Latin America* were completed in early 1986, just after the announcement of the Baker Plan. The balance in these diagnoses of the debt crisis tilts toward flaws in the international financial system, but the authors pointedly assign a large share of the responsibility for establishing a new and more successful development path with Latin American governments. Although the International Monetary Fund and its conditionality are the target of several of these authors, what is most striking about this volume is an overarching, if vague, consensus on future economic development. It is a consensus framed in terms of avoiding the disastrous policies of the past. As Alejandro Foxley describes it, Latin America should aim for a pragmatic course centered on "decentralized development with social pacts and concerted action and less . . . on an omnipresent State or on the advocacy of unrestricted free markets" (p. 89). As Ffrench-Davis complains, however, such a negative definition indicates the "lack of systematization and political consistency" in heterodox economic programs in Latin America (p. 274). Although "pragmatism" may be an advance over the fiscal disasters of statism or the ideologically driven excesses of monetarist experimentation, it offers few concrete guideposts to societies in the throes of political transition.

A few of the authors specify the economic uncertainties that make the transition even more difficult. No economic panaceas are in evidence. Eduardo White makes short work of those who urge reliance on foreign direct investment, which had declined in importance even before the onset of the debt crisis. David Ibarra emphasizes the difficulties in changing development strategy without inflicting insupportable adjustment costs on populations already suffering from years of austerity.

The country case studies that close this volume underline the ambiguities in deriving lessons from the recent past. Dias Carneiro's optimistic prognosis for Brazil was made before the Plan Cruzado's collapse into rising inflation; Peru's situation has also worsened under Alan García, beyond the string of externally and internally induced disasters ably documented by Richard Webb. Costa Rica's successful stabilization—presented by a key player in the stabilization drama, Carlos Manuel Castillo—was unusual in the massive support provided by the United States and the temporary nature of the inflation that was being curbed.

Only an echo of the dramatic political changes taking place in Latin America during the 1980s can be found in these volumes. Political determinants of bargaining in debt negotiations, political constraints on economic policy and development strategy, and the political consequences of transitions from authoritarian rule are definitely secondary. Like so many policy-oriented volumes by economists, the analysts largely set aside the international and domestic political obstacles to their recommendations. Yet one senses that any denouement of this long-running economic crisis will be more influenced by those political changes than by the economic parameters emphasized in both of these volumes.

MILES KAHLER

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The Soviet Union and National Liberation Movements in the Third World. By Galia Golan. Boston, MA: Unwin Hyman, 1988. 368p. $49.95.

The Soviet Union and Revolutionary Warfare: Principles, Practices and Regional Comparisons. By Richard H. Shultz, Jr. Stanford, CA: Hoover Institution Press, 1988. 283p. $25.95 cloth, $16.95 paper.

Among the many issues in Soviet-Third World relations commanding scholarly and policy-making attention since the early 1970s, Soviet ties with national liberation movements (NLMs) have ranked high in priority. This comes as no surprise because at the outset of the 1970s Moscow was backing an impressive range of NLMs, especially in sub-Saharan Africa and the Middle East. While Soviet-backed NLMs have been discussed in scholarly research and policy analyses, no works have systematically studied Soviet perceptions and practices regarding NLMs per se. Galia Golan and Richard Shultz, Jr. seek to fill that void, but approach their subjects with sharply differing research agendas. Golan sets out to understand Soviet theory and perceptions of NLMs by rigorously examining the writing and speeches of Soviet leaders, academics, and lower-level party and government officials. Her efforts reveal the complexity and multidimensionality of Soviet thinking and practices regarding the NLMs from five perspectives: origins; composition, leadership, and organization; ideology; strategy and tactics; and relations with the Soviet Union and the socialist world. The author utilizes primary sources to explore these five issues and then examines patterns of Soviet behavior toward NLMs, drawing essentially on secondary materials. This path-breaking study is akin to previous works that have systematically assessed Soviet literature on the Third World, such as Hough's *The Struggle for the Third World* (1986) and Valkenier's *The Soviet Union and the Third World* (1983).

Shultz, like Golan, is looking at NLMs from a time perspective essentially during the 1970s and early 1980s. But rather than focusing on the diversity and complexity of Soviet thinking about NLMs, his book is a study of revolutionary warfare as Moscow "sharpened, integrated, and orchestrated in a more sophisticated fashion" its political and paramilitary techniques to assist "revolutionary insurgent movements" (p. 45). These policy instruments have included, among other activities, foreign propaganda, international front organizations, arms transfers, training insurgent forces, providing internal security for Soviet clients, and using "surrogate" assets like Cuba and Vietnam. For those interested in how Moscow has combined state-to-state relations in the past with a second-track policy utilizing "active measures" and "low-intensity warfare" in backing revolutionary movements, the Shultz study is a useful companion book to such works as Rice's *Wars of the Third Kind* (1988), which focuses on internal control of revolutionary movements as opposed to Shultz's external ties, and Klare and Kornbluh's *Low Intensity Warfare: Counterinsurgency, Proinsurgency, and Antiterrorism in the Eighties* (1988), which identifies Third World insurgencies as the predominant threat to U.S. security in years ahead. Shultz makes his case by providing a comparative perspective of Soviet relations with Vietnam, the PLO, SWAPO, and Soviet-Cuban relations in Central America. Unlike Golan's work, however, the

Shultz study tends to rely heavily, though not exclusively, on secondary sources.

Apart from the different research agendas of these books, they reach surprisingly opposing conclusions on Soviet thinking and policies regarding key issues in Soviet relations with NLMs. Shultz treats Soviet approaches to NLMs as representing a unitary or monolithic "Soviet view" that has evolved through stages, dominated by a kind of orthodox outlook advocating his "revolutionary warfare" approach. Golan emphasizes the complexity of Soviet schools of thought divided into orthodox thinkers and the nonorthodox thinkers whose ideas have waxed and waned since the last years of the Brezhnev regime but whose views now dominate Gorbachev's "new political thinking" in international relations. While nonorthodox thinkers, Golan argues, hold various views regarding her five categories, they have been pragmatic and realistic in assessing obstacles to socialist transformation through backing of NLMs, rather than assessing NLMs as an easy road to undermine the West.

On questions of guerrilla warfare and terrorism, Golan and Shultz again arrive at different conclusions regarding Soviet perceptions and policies. Golan argues—and uses credible evidence to make her case—that many Soviet analysts have been highly dubious about the revolutionary potential of the peasantry and guerrilla warfare and have shown a striking ambivalence regarding armed struggle. Shultz's formula for "revolutionary warfare," on the other hand, argues that backing for armed struggle and guerrilla warfare has been more of a clear-cut routine affair in Soviet policy.

Soviet support of terrorism—that ever-so-contentious subject in Soviet-Third World studies—receives remarkably opposing treatment in these books. Golan uses weighty evidence to argue that conventional warfare has been preferred over guerrilla warfare and sabotage over terrorism. Shultz, on the other hand, refers to Soviet use of various tactics "ranging from threats to the use of terrorism" (p. 5) and Moscow's training of "insurgents and terrorists" (pp. 39, 141). Golan's more careful use of primary evidence and definitions and Shultz's tendency to put insurgents and terrorists together without making clear what he means by *terrorism* or *terrorists* leads one to listen more to Golan on this matter.

Golan's book is truly outstanding and should be read by those who follow Soviet-Third World relations and Soviet foreign policy in general. Her breakdown of NLMs into anticolonials and secessionist movements; identification of contending orthodox and nonorthodox schools of thought in party and government organizations; emphasis on pragmatism and caution in Soviet approaches to NLMs; clarity on the subjects of guerrilla warfare, armed struggle, and terrorism; and discussion of how the Soviets behave both before and after independence is achieved by an NLM—all based on original research and thought—make this a major contribution to the field. It provides essential backdrop reading for a fuller understanding of how, and in what ways, Soviet policy has changed under Mikhail Gorbachev and the origins and evolution of the nonorthodox kind of thinking about NLMs that now appear manifest in his approach to the Third World.

Although Shultz's work falls less into the category of original research, it still provides a comparative study of how the Soviets behaved in backing revolutionary warfare during the 1970s and early 1980s. While Shultz tends to stretch his evidence in sections of the book in ways that fly in the face of rapidly breaking events under the Gorbachev leadership—stating, for example, that Moscow "is extremely pessimistic about any solution (in Namibia) short of armed revolutionary struggle" (pp. 123–24) when Gorbachev has just pressured Angola and Cuba into a politically negotiated agreement for a Cuban withdrawal from Angola and Namibia's independence in 1989—his work nevertheless tells us how the Soviets have acted in the past.

These books should be read in the context of the remarkable changes in Soviet thinking and policies toward the Third World and NLMs since Gorbachev came to power—downgrading support of NLMs, deideologizing foreign policy, reducing military competition with the United States in Third World arenas, managing an "interdependent" global system, lowering the costs of foreign involvements, pressing NLMs in power to pursue more efficient economic management, and negotiating political settlements to regional conflicts. Gorbachev's "new political thinking," with its shift away from angling for spheres of interest in the

Third World and looking toward increased superpower cooperation in managing regional conflict, suggests that Golan's nonorthodox thinkers now are the driving force in Soviet relations with NLMs. This pattern points to considerable distance from the world described by Shultz in the 1970s and early 1980s, although some remnants of previous policy—the desire not to abandon positions gained in places such as Angola, Mozambique, and Ethiopia totally—may remain, at least until either costs simply become too great or local forces for change make the Soviet presence irrelevant.

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China and Northeast Asia: The Political Dimension. By Harry Harding. New York: University Press of America, 1988. 100p. $15.75 cloth, $5.25 paper.

China and Northeast Asia shares a theme prevalent in much recent scholarship on China's domestic as well as foreign politics, which stresses a new era of reform since the passing of Mao Zedong. Regarding Beijing's foreign relations, Harding argues that this new era "blends peaceful coexistence, economic cooperation, and diplomatic flexibility together with territorial, economic, and strategic nationalism" (p. 16). Instead of revolutionary fervor and ideological challenge to the existing international order, Beijing has taken a pragmatic turn in its foreign policy.

Harding presents a succinct and balanced account of the nature of, and prospects for, Beijing's relations with its neighbors in Northeast Asia. He shows that in recent years Sino-Soviet relations have entered a period of detente and Sino-U.S. relations have undergone substantial consolidation, even though in both cases divisive issues (e.g., Cambodia, Taiwan) remain as irritants. During the 1980s, a close economic and diplomatic relationship has also been developing between Beijing and Tokyo, so that their ties are more cordial now than at any other time in recent memory. As another contribution to a more relaxed climate for Northeast Asia, China has apparently been exerting its influence in maintaining stability on the Korean peninsula. Finally, Chinese policies have continued to strive for national reunification, achieving greater success in negotiations with Britain and Portugal for the reversion of Hong Kong and Macao than in motivating the nationalists on Taiwan. In short, there is a sense that even though the spirit of Camelot is still barely visible, the spirit of Hobbes has at least greatly abated in the region.

The book is therefore generally upbeat in its assessment of the direction of Chinese foreign policy and its implications for the region's and China's stability and prosperity. A basic premise is that Beijing has given top priority to domestic economic development, which in turn necessitates a peaceful and conducive international environment. Foreign technology, expertise, capital, and trade are assigned an important role in Deng Xiaoping's economic reforms. China is thus becoming a more stable, secure, and "responsible" international actor. It also seems poised for rapid economic growth and perhaps even political liberalization.

Recent events in Beijing took a sharp and disturbing turn, with the government using military force to crush student demonstrators agitating for political reform. By undertaking this bloody suppression, the regime seriously undercut its domestic credibility and international reputation. Deng Xiaoping's professed goals of economic modernization, national reunification, and orderly transfer of power appear to be in grave jeopardy. China's foreign relations, at least in the short run, face considerable uncertainty and turmoil, as military sales, economic projects, cultural exchanges, and diplomatic contacts with other countries are being suspended or canceled as a result of foreign reactions of shaken confidence and moral outrage to Beijing's crackdown. It appears that as on several previous occasions (e.g., Beijing's intervention in the Korean war, its split with the USSR, the Cultural Revolution), China has again confounded scholarly and official expectations in the West. Whereas most foreigners have been concerned with the possible long-term destabilizing effects of a *strong* and hegemonic China in regional affairs, the June 1989 events warn that the prospect of a *weak* and unstable China presents a more real danger in the near term.

STEVE CHAN

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Reasons of State: Oil Politics and the Capacities of American Government. By G. John Ikenberry. Ithaca, NY: Cornell University Press, 1988. 232p. $32.95 cloth, $10.95 paper.

In *Reasons of State* John Ikenberry assesses U.S. domestic and international energy policy formation, transformation, and implementation during the turbulent 1970s. The analysis is grounded in the statist literature. Ikenberry argues that U.S. "policy responses to the oil shocks were shaped and channeled by the institutional structures of the state" (p. x). He contends that existing institutional structures constrain the creativity, flexibility, and choices available to leaders seeking solutions to pressing problems, concluding that the U.S. government's inability to craft and implement enduring solutions to critical problems has promoted reliance on the market as an alternative to government intervention. For Ikenberry, the market is an important instrument of state capacity.

His critique of the statist literature questions whether the relative strength of the state is the main determinant of state power and control over economic and political outcomes. He suggests that both strong and weak states are tied to past policy commitments. States with a tradition of intervening in the economy and society may have less freedom to change direction rapidly in crises. Indeed, "the flexibility of state action . . . may be just as important as the degree of state control of the economy and society or the level of the organizational development of the state" (p. 206). Deregulation ("imposing the market") is seen as a viable alternative to direct intervention as a mechanism for exerting state power.

Ikenberry asks why, in contrast to many other industrial countries, the United States initially responded to the oil shock of 1973–74 by seeking an ambitious, international cooperative response. He then explores the reasons that U.S. officials shifted from international to domestic, and from interventionist to market-driven, policies. He argues that the failure of the United States to establish a comprehensive national energy bureaucracy before the oil shocks limited its ability to respond to the crises of the 1970s. Therefore, the United States was forced to seek international cooperation rather than seeking domestic solutions to the problem.

To support his interpretation, Ikenberry starts with a historical overview of the evolving organization and structure of the U.S. government with particular reference to energy policy. He then considers the Organization of Economic Cooperation and Development's response to the Arab oil embargo of 1973—contrasting Kissinger's multilateral cooperative strategy with European and Japanese bilateralism. Although the United States-sponsored International Energy Agency (IEA) developed emergency oil-sharing and information-sharing functions, the United States failed to gain agreement to use the IEA to confront OPEC.

The story next turns to the largely unsuccessful attempts by U.S. "officials to redefine the state's role in the energy sector" (p. 104). The increased use of energy research and development funding as an instrument of government policy during the 1970s is examined. Ikenberry argues that the state, unable to gain control over energy developments, responded by adopting "spending programs designed to alter and encourage private behavior" (p. 138). He concludes by arguing that the Carter administration decision to use the market to achieve the state's goals was the result of a transnational bargain. At the 1978 Bonn Summit Chancellor Schmidt, in effect, pledged to reflate the German economy in return for President Carter's commitment to ease controls over U.S. domestic petroleum.

Overall, *Reasons of State* is a bold and provocative book rich in ideas and insights. The analysis and scholarship are detailed and careful. Ikenberry tells a fascinating story that should provoke and stimulate readers from several disciplines. But there are areas where the analysis is not always convincing. For example, the market is undifferentiated. All energy companies are treated the same. They are assumed to act identically. But Exxon is not Occidental Petroleum, and British Petroleum and ENI may act quite differently. Moreover, the analysis is so concentrated on the U.S. side that the story lacks comparative perspective. In addition, by focusing on cooperation in the commercial sphere, Ikenberry may underestimate the cooperation achieved in the allocation of energy to meet military needs. Yet cooperation on the security side is precisely where the statist approach should be strongest.

A crucial issue is how important the bargain struck at the Bonn summit was in converting the Carter administration to a more free-market approach. Although Ikenberry acknowledges disagreement among the participants about the significance of the deal, his argument about its galvanizing impact is not totally convincing. Other factors leading to the change in policy receive too little attention.

Still, although disagreements with his interpretations are likely, other scholars concerned with the statist literature and with U.S. energy policy will want to deal with the ideas presented in *Reasons of State.*

JONATHAN D. ARONSON

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Crisis and War. By Patrick James. Montreal: McGill Queen's University Press, 1988. 200p. $32.95.

In 1981 the subdiscipline of war-peace studies was shaken up by the publication of Bruce Bueno de Mesquita's *The War Trap.* Applying statistical methods to 215,000 possible interstate wars and 76 actual wars (1823–1974), he argued that leaders initiating war act as if they are expected-utility maximizers, with *probability of success* defined as national capacity discounted by geographic distance and *utility* defined as the degree of conformity of other nations to the alliance policies of their own. Many critics contested the definitions, but the theory made successful and surprising predictions. It delivered the goods, at least in statistical terms. Advocates of balance-of-power theories or misperception theories of the Jervis-Stoessinger sort were thrown on the defensive.

Patrick James's new study provides us with the deepest assessment yet of Bueno de Mesquita's methods and conclusions. James analyzes 175 international crises (1948–75) and a subset (crises that evolved into wars), seeking necessary and sufficient conditions for war precipitation. The use of relatively contemporaneous events will come as a relief to those worried about the wide time span of Bueno de Mesquita's study, and James's testing for sufficient conditions is an improvement over Bueno de Mesquita's theses, which state necessary conditions and make the actual initiation of war something of a mystery.

James's statistical tests show that expected utility maximization is a statistically significant factor differentiating resolved crises from crises that precipitate wars. But he demonstrates that the presence of multipolar power configurations is also a statistically significant factor, reviving a thesis first stated by Richardson and popularized by Waltz. Most remarkably, his tests reveal that degree of internal conflict is a significant factor in war precipitation, resurrecting the "safety valve" thesis most war theorists had long since left for dead. Indeed, James's regression analysis of these three hypotheses reveals that internal conflict is statistically the strongest factor, though the conjunction of the three is significantly more predictive than any one by itself. Certainly, these results cast doubt on the priority Bueno de Mesquita assigns to lack of utility maximization as an explanation of why nations *don't* go to war.

James's study sets new standards for rigor in aggregative analyses, and he has developed and tested his hypotheses with great care. Nevertheless the results that emerge can be no better than the data that go in, and on this score some doubts arise. The coding on the crisis list is often controversial. Even if we code the United States as a third party in the Gulf of Tonkin incident, it is hardly obvious that North Vietnam initiated this crisis, given coastal raids by South Vietnam in late July 1964. What is needed is a more detailed commentary on the crisis list, together with a sensitivity analysis indicating how the statistical results would change, given various alterations in the coding.

Furthermore, Lewis takes over the utility measures developed by Bueno de Mesquita, and one might wonder whether these are the most useful that might be employed. Do leaders who initiate wars always do so in hopes of swinging opponents to their point of view? Certainly, some want their opponents not complaisant but dead. Can a psychologically realistic measure of utility treat losses as numerical negations of utility gains? Perhaps the risk of war is more often taken in fear of loss than hope of gain. Each question presents options, and the options deserve to be tested. This is one of the rare books that would have been better if it was longer.

There are some case studies in this book, but not many; and historically inclined students of the subject will object that the emphasis on aggregates misses the trees for the forest. Given the number of false alarms that every hypothesis tested by Lewis generates, the statistical results presented here will hardly support lawlike conditionals. As a result, we may have gained knowledge about wars in general without gaining insight into any particular war. It is too early to tell whether the research program inaugurated by *The War Trap* will flourish or degenerate, but studies like this are exemplars of their kind.

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Surprise Attack: The Victim's Perspective. By Ephraim Kam. Cambridge: Harvard University Press, 1988. 304p. $25.00.

Because the initiator of a surprise attack must engage in certain subtle battle preparations, almost all subsequent governmental inquiries struggle with the question of why these warning signs were not heeded and the appropriate defensive measures not undertaken. Why were the danger signs ignored?

The motivating question of *Surprise Attack* is, therefore, how could a government and its intelligence apparatus fail to detect impending hostilities? Although sometimes the victim intentionally abstains from taking the appropriate actions for fear of increasing tensions that might lead to war, Kam's premise is that inadequate military responses are due principally to distortions in the decision-making process. The failure to detect the war preparation can be traced to four functional levels of the governmental decision-making process: the individual military analyst as a source of misperception and judgmental bias, problems associated with small group decision making, the larger structure of the intelligence community and its reaction to the imminence of war, and the relationship between the intelligence community and top governmental elite. Although there are a number of other factors that might cause a government to abstain from defensive preparations (such as the costs of mobilization), Kam views the decision-making process in the intelligence community as central to understanding how an impending attack goes undetected.

The book opens with a useful and thoughtful conceptual discussion concerning the nature of surprise attacks. There are four questions that the intelligence community must answer, which, when answered incorrectly, make a surprise attack possible. The first is *whether* an attack will occur at all. This is a rare occurrence and evident only in those cases where there was no prior history of hostilities (as in the German invasion of Norway during World War II). More common is a failure to detect *when* the attack will occur (as in the Yom Kippur War), *where* the attack will occur (as at Pearl Harbor), and *how* the attack will occur (as in the 1956 Sinai War).

The remainder of the book applies some of the decision-making literature to the problem of strategic estimation in general and detecting war in particular. Because most discussions of the intelligence community tend to be either case-specific or anecdotal, Kam's theoretically focused discussion is welcome; however, a greater emphasis on the nature of the intelligence community itself and less on reviewing the vast decision-making literature would have significantly strengthened the remainder of the book. As it stands these sections present almost every imaginable hypothesis concerning information processing at the individual and group level. This rather tedious discussion supports two observations. The first, implicit, observation is that although the intelligence community might be distinct because of its governmental function and the individuals that staff its offices, it is susceptible to the same individual, organizational, and bureaucratic problems that are present in other branches of government. The second, explicit, conclusion is that detecting a surprise attack, a type of strategic estimation, "involves many interdependent analytical difficulties and judgmental biases, organizational obstacles, and political as well as military constraints. . . . Indeed, it is the very complexity of the surprise attack that renders it so difficult to predict and prevent"

(p. 2). Consequently, the causes of surprise attacks are overdetermined, since no single factor (such as intelligence failure or bureaucratic politics) can capture the complexity of the issue. The book, therefore, concludes on a correspondingly pessimistic note: "History does not encourage potential victims of surprise attack" (p. 233).

An interesting observation by Kam is that a principal source of intelligence failure is an overemphasis on the enemy's capabilities at the expense of enemy intentions. This occurs because most analysts are better equipped to gauge arsenals than strategic intentions. This conclusion is consistent with some evidence about the causes of deterrence failure. As Richard Ned Lebow has argued in *Psychology and Deterrence* (1985), attackers do not jump through "windows of opportunity." Rather, international hostility is more often initiated because of perceived need, not because of the failure of the victim to adopt a suitable deterrence posture. (That capabilities are not as critical for understanding war initiation might help to explain why most initiators of surprise attacks eventually lose the war.) Kam's findings, therefore, support recent psychological approaches to deterrence failure, indicating that military and scholarly analysts alike would do well to focus on the opponent's motivations rather than solely on its capacity to attack.

This book could have been strengthened in a couple of ways. First, while Kam draws on a variety of cases of surprise attack since 1939 in order to support his hypotheses, these cases, despite the claim that this is a comparative case study, serve mainly an anecdotal function to support many familiar hypotheses concerning the sources of misestimation. A more useful research strategy might have been to provide a more extended and richly textured analysis of a few carefully selected cases (particularly since certain cases, notably the Israel-Arab wars and Pearl Harbor, receive overwhelming attention). This would have demonstrated more clearly both that the problems at various functional decision-making levels contribute to intelligence failure and that surprise attacks are in fact overdetermined.

Second, the conceptual discussion of surprise attacks presented in section 1 could have been more fully integrated into the rest of the book. For instance, the analysis of the aspects of surprise attacks (whether, when, where, and how) remains unconnected to the subsequent discussion of the victim's failure to take adequate preparations. Linking aspects of surprise attacks to various sources of intelligence failure might have made it possible to examine, for instance, whether the failure to detect where an attack will occur is more often rooted in the military intelligence community itself than the failure to detect whether an attack will occur, which may in turn be more dependent on top-level decision makers or their interaction with the intelligence community.

Despite these weaknesses, *Surprise Attack: The Victim's Perspective* is a welcome addition to the literature on strategic estimation and the failure to detect the likelihood of war and is particularly valuable for its application of decision-making theory to an examination of the intelligence community.

MICHAEL BARNETT

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Imperial State and Revolution: The United States and Cuba, 1952-1986. By Morris H. Morley. New York: Cambridge University Press, 1987. 575p. $59.50 cloth, $16.95 paper.

From Confrontation to Negotiation: U.S. Relations with Cuba. By Philip Brenner. Boulder: Westview, 1988. 120p. $24.95 cloth, $7.95 paper.

Cuba and the United States have long eyed each other warily, with a mixture of suspicion and circumspection, with distrust and detachment. Proximity alone explains much, and a great deal can be understood by geography. Cubans probably know the U.S. nation, and are known by them, better than do any other people in Latin America. When both were colonies of Europe, circumstances had created needs in each that only the other could meet. Proximity also shaped expectations, and in the United States the expectation took hold early that destiny had manifestly meant for Cuba to be part of the union. It was simply inadmissible if not inconceivable that Cuba, so near, so strategically situated, could somehow remain outside the sphere of U.S. control.

Proximity also shaped expectations in Cuba. Certainly many Cubans endorsed the proposition of attachment to the United States. But many more did not. The determination in Cuba to pursue a separate and sovereign nationality was also itself in no small way shaped by proximity. Cubans came to know the United States well enough to know they did not want to become part of it.

This tension has been a constant element in nearly two centuries of Cuban-U.S. relations. It has surfaced on a recurring basis and in a number of forms. U.S. resolve to control Cuba and Cuban determination to resist control have become part of the character of each and something of an obsession of both.

This was true before 1959, and nothing has occurred since then to suggest that the preoccupation of each with the other will subside anytime soon. That a small client state managed to disengage itself from U.S. control and, more, unabashedly to embrace Marxism-Leninism and thereupon proceeded to align itself with the Soviet Union—this only 90 miles offshore in a region hitherto secure as sphere of influence—provided the stuff of high drama and enduring controversy.

The Cuban challenge to the premises and propriety of U.S. hegemony and the response of the United States to that challenge have served as the twin themes of a vast literature since 1959. Journalists and novelists, polemicists and scholars, ex-diplomats and former politicians from both sides have all made contributions to this vast body of literature. In general terms, it is commonly known that the United States has opposed the Cuban revolution and that after nearly three decades, socialism in Cuba remains for U.S. citizens as inadmissible today as it was the day that Fidel Castro proclaimed the Marxist-Leninist character of the revolution.

Each book under review makes an important contribution to a deeper understanding of the character and consequences of U.S. hostility toward Cuba. Read together, both books make for sober reading indeed and underscore the implacable adversarial character of U.S. policy. Morris H. Morley's *Imperial State and Revolution* examines themes long familiar to observers of U.S. foreign policy and Latin American affairs. Never before, however, have the individual acts and multiple patterns of the U.S. determination to destroy the Cuban revolution been presented with such compelling clarity and overwhelming detail. Based on previously classified records obtained through the Freedom of Information Act and on interviews with officials high and low, Morley chronicles the many ways the United States (sometimes by itself but just as often in concert with ally countries in Europe and client states in Latin America) plotted against the revolution. Propaganda campaigns and psychological warfare, subversion and sabotage, diplomatic isolation and economic blockade, assassination attempts against Cuban leaders and attempts to destroy the Cuban economy, and one-armed invasion and repeated covert assaults are only some of the means employed by the United States in the effort to crush the revolution—sometimes in sequence, sometimes all at once. Philip Brenner's *From Confrontation to Negotiation* corroborates from the larger policy context the approaches Morley details at an operational level. Brenner provides an overview of the national and international dimensions of political relations between both countries over the last 30 years.

While both books are critical of U.S. policy toward the revolution, the differences of emphasis in each are sufficiently great to underscore the difference in perspectives that have long characterized the discourse on Cuba. What Brenner identifies as "confrontation" Morley calls "warfare" and "vendetta politics." Brenner operates out of a liberal framework in which the issue of communism and Cuban relations with the Soviet Union are the central elements to U.S. policy formulations. Morley's arguments are derived from a radical perspective in which the issue of Cuban communism is certainly a factor but is neither intrinsic nor essential to the character of the U.S. policy. To simplify their respective positions only slightly—for Brenner U.S. hostility to communism led to problems with the revolution, and for Morley U.S. hostility toward the revolution led to problems with communism. For Brenner the critical point is a breakdown of negotiation, and the optimum denouement for the United States should be reconciliation with the revolution. For Morley the central question is the breakup of empire, and the ultimate goal of the United States is the destruction of the revolution.

Suggested in the Morley book and all but absent in the Brenner study is the implication of the larger function of the U.S. hostility to the revolution. Indeed, the issue of Marxism-Leninism in Cuba has less to do with bilateral relations than with the defense of U.S. hegemony in Latin America. The Cuba policy of the United States functions in a larger context, and in this instance Morley may be closer to the mark than Brenner. For three decades the United States has used Cuba to demonstrate the high cost of socialism in Latin America. The policy has been largely a punitive one, an object lesson to other would-be socialist governments—really nothing less than a state siege to make the cost of revolution so unbearable as to deter and discourage others from emulating the Cuban example. The logical conclusion of Morley's arguments is sound. The United States cannot normalize relations with Cuba without seeking first to exact from Havana some significant concession—not, at least, without fear of sending the wrong "message" that Washington will eventually reconcile itself to Marxism-Leninism in the Western Hemisphere.

Both Morley and Brenner make important contributions to the ever-growing literature on Cuba. Both present their arguments in cogent and often compelling form. Neither book can boast of elegant prose or methodological advances. Brenner provides little by way of new information. Morley provides new information but often declines to identify his sources. Important information is attributed variously to an anonymous "state department official," an unnamed "national Security Council staff member," or an unknown "Department of Commerce official." Anonymity may indeed have been a condition of the information, of course. Nevertheless, it is always unedifying to deny a reader the opportunity to assess the worth of information as a function of its source. In balance, however, both books are welcome additions to the literature and promise to provide the basis for continued discussion about the latest manifestation of what has been known for almost two hundred years as the "Cuba question."

LOUIS A. PÉREZ, JR.

University of South Florida

Diplomacy and the American Democracy. By David D. Newsom. Bloomington: Indiana University Press, 1988. 160p. $25.00 cloth, $9.95 paper.

This book by a retired U.S. diplomat attempts to answer the fundamental question why U.S. foreign policy has not been particularly successful in recent decades. It emphasizes perceptual differences between the United States' view of the world and the rest of the world's view of the United States and tries "to help us see ourselves as many see us" (p. xi). Toward this end the study discusses the changing nature of traditional diplomacy; its relationship to the State Department, Congress, the media, intelligence, and the military; and special challenges posed by the Third World, human rights, political change, terrorism, and foreign aid.

On the plus side, the book is extremely well written and full of interesting anecdotes culled from years of first-hand experience. It is quite even-handed in its judgment of controversial issues and is often astute. For example, it nicely identifies the U.S. perspective on diplomacy as "a mixture of ignorance of its details, suspicion of its objectives, contempt for its importance, and fascination with its romance" (p. 6). Two of its best sections deal with a taxonomy of types of foreign policy leaks to the press (pp. 57–58) and a summary of the contrasting contexts of the U.S. achievement of independence in 1776 and the Third World nations' achievement of independence in the 1950s and 1960s (pp. 79–82).

However, this work is disappointing because it says very little that is new or innovative. Most of the insights ring true but familiar. Too much space is devoted to describing and rationalizing diplomatic complexities, and too little space to analyzing and dissecting them. There is virtually no reference to (or evidence of familiarity with) social science findings on topics discussed, an omission that is particularly troublesome when dealing with revolutions and coups d'etat, topics where excellent theoretical and empirical studies exist.

Furthermore, the book seems dated in dealing with major global trends. The two major political developments deemed to have the most crucial impact on changing the style of diplomacy after World War II are the process of decolonization and the confrontation between the Soviet Union and the West (p. 3). While these developments were undoubtedly important, more recent international shifts, such as the rise of the Pacific Rim and the newly unifying European Community, receive little notice. The work uses formal embassy-based diplomacy as the central lens through which all aspects of foreign policy are scrutinized even though the author himself admits that this traditional form of diplomacy (as opposed to newer forms of negotiations) has declined significantly in importance in recent years. More specifically, the chapter on intelligence seems especially behind the times—for example, the claim that "the CIA is charged primarily with following the activities of Soviet and satellite embassies" in each nation (p. 139) is not nearly as true as it once was.

Finally, the book is remiss in providing no concrete policy recommendations at the end. The one general prescription provided is a change "in the attitude toward other nations and their interests on the part of the American people and leaders" (p. 218) in the direction of increased sensitivity and attentiveness to cultural differences, advice too vague and too trite to be meaningful. What this book's discussion appears to call for most urgently is a critical reexamination of the prevailing balance in the United States between the dictates of diplomacy and the dictates of democracy, undertaken in the explicit context of an increasingly competitive world that the United States can no longer realistically hope to dominate.

ROBERT MANDEL

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The Sword and the Dollar: Imperialism, Revolution, and the Arms Race. By Michael Parenti. New York: St. Martin's, 1988. 206p. $16.95 paper.

This brief text examines selected aspects of U.S. foreign policy—in particular, policy toward the Third World and relations with the Soviet Union. Parenti argues that a "major component" of U.S. foreign policy is the "defense of imperialism" (p. 106), which he defines as the "forceful expropriation of one country's land, labor, markets, and resources by another" (p. 11). Parenti argues that contrary to the view of many observers, U.S. foreign policy is "quite rational in its goals . . . resourceful in its methods" and "usually effective in its undertakings" (p. 4).

The most valuable portion of the text is the part that focuses on U.S. policy toward the Third World and Third World revolutions. Since economic interests in the United States are forced to expand, the Third World serves as an attractive source of labor, raw materials, and markets. The imperialists are able to exploit the weaknesses of Third World societies to their advantage and manifest great hostility to popular revolutions in those dependent societies. In this portion of the text, Parenti develops several interesting concepts. He describes the U.S. empire as a "condominium empire" (p. 66); that is, the United States does not administer its colonies directly (as an apartment owner might) but, rather, grants them independence, so that their leaders must pay the costs of security, and so on (as the owner of a condominium would) while U.S. firms retain ownership of the productive resources. The author also provides much supportive data to buttress his assertions. For instance, to support his claim that foreign investment does not serve the interests of the majority of citizens of Third World states, Parenti notes that during the "Brazilian miracle" of the 1960s and 1970s, the GNP tripled while the real income of the poorest 80% of the population declined by half.

The portion of the text examining Soviet-U.S. relations and the arms race is less valuable. The critique is not related to the basic theme of the book. Parenti does remind us that Soviet-U.S. hostility predated 1945, and he relates U.S. hostility to the conclusion of a successful socialist revolution in the USSR. The assessment of relations in recent times focuses largely on nuclear weapons issues and the development by the United States of a first-strike capability.

Parenti does not argue that such economic factors explain all of U.S. foreign policy, but the existence of noneconomic factors "does not compel us to dismiss economic realities" (p. 127). These noneconomic motives, for instance, nationalism, the desire for control, and militarism "usually do not compete against economic class motives but dovetail with them" (p. 131). Parenti does not explain why, if these variables are mutually supportive, economic motives are fundamental.

The emphasis on one primary explanatory variable leads, perhaps inevitably, to exaggerations and distortions. Thus, Parenti's focus leads him to exaggerate U.S. behavior and its consequences and create double standards to justify similar behavior by the Soviet Union. He provides extensive documentation of U.S. opposition to Third World revolutions, but overstates his case when he asserts that the United States has "overthrown a dozen or more popularly elected, left-reformist democracies" since World War II (p. 191). Similarly, he argues that "many—but certainly not all—of the grotesque distortions of the Khmer Rouge rule [in Kampuchea] have their roots in the U.S.-sponsored war that came before" (p. 124).

Parenti's defense of antiimperialist revolutions also results in double standards. For instance, while U.S. interventions in the Third World are criticized, Parenti does not condemn similar Soviet behavior in Afghanistan. The reason is that the United States intervenes "to prevent fundamental changes in social relations and class structures of Third World countries," while the Soviets have intervened "to support the forces that were promoting such changes" (p. 188). Similarly, the Cubans are praised for their intervention in Angola "to defend a revolution that was being threatened by reactionary forces" (p. 188). He is also very critical of recent U.S. attempts to develop a first-strike capability, while ignoring comparable efforts by the USSR.

These criticisms notwithstanding, this is a useful text. It is a clear, concise statement of a tradition in foreign policy analysis that is too much ignored. Whatever the relative importance of economic motives for U.S. policy, it is undoubtedly true that economic factors do play at least some part in some foreign policy decisions. Parenti is correct when he asserts that such motives are often ignored by analysts of U.S. policy. This text is a useful corrective to that omission.

DAVID T. JERVIS

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The Indochina Tangle: China's Vietnam Policy, 1975-1979. By Robert S. Ross. New York: Columbia University Press, 1988. 392p. $40.00.

China's punitive invasion of Vietnam in early 1979 provoked a rash of studies on security strategies and foreign policies of key actors in the Indochina conflict. Some focused on Vietnam's aims in Cambodia, others on the intrusion of Sino-Soviet rivalry into regional affairs. Robert Ross's contribution, a dissertation revision, is a meticulous analysis of China's policy toward Vietnam in the four years between Vietnam's reunification in 1975 and China's self-defensive counterattack.

The book also affirms the thesis—advanced by Peter Van Ness in *Revolution and Chinese Foreign Policy* (1970)—that China's policy toward a country is determined more by that country's relations with Beijing's "principal enemy" than by its revolutionary or ideological character. Because Beijing identified Moscow as its chief adversary in this period of adjustment to a new regional balance of power, the "most urgent aspect" of its policy toward Vietnam was to avert Soviet encirclement. Presumably, Vietnam's postwar behavior would have been a minor irritant for China had Beijing not perceived Soviet influence behind it. Ross develops this argument in a sustained, rigorous, and persuasive manner.

China's immediate postwar aim, Ross maintains, was not to eradicate Soviet influence in Indochina—a clear impossibility—but to discourage Vietnam from developing still closer relations with Moscow. At first Beijing acted with a restraint in order not to drive Hanoi into Moscow's arms and attempted to ameliorate the tensions between Vietnam and Pol Pot's regime in Phnom Penh. However, China's succession politics and favorable response to Cambodian demands for closer relations aroused Vietnamese fears of Sino-Cambodian collusion. China also declined to compete for influence in Hanoi by increasing economic assistance, for this was competition that only the Soviet Union could win. As Vietnam turned toward the USSR for money and reassurance, Beijing's displeasure deepened. Finally, Ross suggests, U.S. awakening to Soviet "hegemonism" in spring 1978 ended Chinese leaders' sense of isolation and helped persuade them to deal forcibly with what they perceived as Vietnamese complicity in Soviet encirclement.

Four irritants specific to Sino-Vietnamese relations, Ross argues, were only catalysts and symptoms of conflict over the larger strategic issue: (1) the border dispute, a minor affair until China insisted on negotiating a treaty to replace the one forged with France in the nineteenth century; (2) friction over the offshore islands, on which China publicly criticized Vietnam only after Hanoi had signed a Treaty of Friendship and Cooperation with Moscow in November 1978; (3) Vietnam's effort to construct special relationships with the regimes in Vientiane and Phnom Penh, which eventually drew China into rivalry with the Vietnamese on the side of the pro-China Khmer Rouge; (4) Vietnam's treatment of its ethnic Chinese minority, which China assailed while ignoring the murder of Chinese in Cambodia. With respect to all of these irritants, Ross points out, China's orientation was a product of its concern for Hanoi's relationship with Moscow; and *this* relationship, not the irritants, provoked China to attack Vietnam in February 1979. Noting that China had previously used force only in situations that threatened Chinese territory or territorial claims, Ross properly concludes that the invasion revealed China's emergence as a regional power with regional interests.

Ross denies that the parochial needs of individual leaders had much more than marginal effect on the pursuit of Beijing's strategic interests. He also asserts "the total irrelevance of ideology as a cause of Sino-Vietnamese conflict" (p. 255). In other words, China was an essentially rational and unitary practitioner of realpolitik in this case. Does this finding apply to China's foreign policy generally? J. D. Armstrong showed in *Revolutionary Diplomacy* (1977) that the balance of ideological and other elements in China's foreign policy has varied greatly, with the Sino-Soviet dispute causing Beijing to turn toward conventional power politics. But Ross's case study approach cannot determine whether Chinese policy toward Vietnam in 1970s was the manifestation of Beijing's socialization to realpolitik in relations with all countries or a unique side effect of its dispute with Moscow. Moreover, the book's thesis, dependent as it is on a "principal enemy," has limited power to explain Chinese policy during detente and reform. As for the discussion of Vietnam, passages that show weak grasp of Hanoi's perceptions and unqualified references to an "Indochina Federation" will remind Vietnam specialists that Ross is a Sinologist. But this book does not pretend to explain the total dynamic of Chinese policy nor to provide deep analysis of Vietnamese responses. It is a study only of China's behavior toward Vietnam in events that culminated in the Third Indochina War. As such, it is thorough, lucid, and reliable.

William S. Turley

Southern Illinois University

Soviet Military Doctrine: Continuity, Formulation, and Dissemination. By Harriet Fast Scott and William F. Scott. Boulder: Westview, 1988. 320p. $44.50 cloth, $19.00 paper.

Soviet Nuclear Strategy from Stalin to Gorbachev: A Revolution in Soviet Military and Political Thinking. By Honoré M. Catudal. Atlantic Highlands, NJ: Humanities Press International, 1989. 400p. $49.95 cloth, $15.00 paper.

What is the significance of formal Soviet military doctrine, and how has it changed in the postwar years? M. S. Gorbachev's comprehensive revision of all aspects of formal Soviet ideology makes these questions particularly timely today. These two books illustrate some of the problems in answering them.

Harriet and William Scott contend that the Soviet Communist party's military doctrine decisively determines the country's military policy. Further, they maintain that this doctrine has undergone only minor change since it was first adopted to the nuclear age in the late 1950s and early 1960s. The Scotts' basic methodology is close scrutiny of Soviet sources, including the speeches of top leaders, Party documents, and specialists' literature dealing with military affairs. This approach has several virtues. It forces the analysis to stay true to Soviet definitions, terms, and periodizations. Soviet doctrine is, after all, a body of published materials; and the Scotts' liberal quoting from original sources conveys a sense of their nature.

On the negative side, the Scotts' approach virtually guarantees that no scholarly cumulation of knowledge will occur; since it ignores the work of other Western specialists on the topic—many of whom forward radically different interpretations of the same material. Though they occasionally refer obliquely to "Western researchers" with differing opinions, in 265 pages of text, the authors cite Western specialists other than themselves by name only twice—and then for points of fact rather than interpretation. It is as if the works of Garthoff, Dinerstein, Erickson, McGwire, Holloway, Vigor, and so on did not exist.

The main problem, however (and it is one common to works on this subject), is that the authors do not offer an explicit standard by which they judge the importance of various Soviet sources. They accept the Soviet definition of *doctrine*, the "military policy of the Party"; yet they treat the statements of top Party leaders unevenly. For some reason, the early, "war-fighting" L. I. Brezhnev is authoritative; but the later "deterrence" Brezhnev is an unreliable source. Why are the speeches of N. S. Khrushchev decisive, but not those of Gorbachev? It is never explained. In discussing recent Soviet doctrinal moves toward "defensive defense," the authors observe that "NATO planners should not forget that change from an 'offensive' to a 'defensive' doctrine was purely nominal, a change in words and nothing more" (p. 258). But *all* doctrine is words! Why are these words dismissed and others accepted? If the Scotts had accorded the same weight to Soviet statements between 1985 and 1988 as they do to those between 1959 and 1962, they would have reached a different conclusion about continuity.

As its title suggests, Honoré M. Catudal's book comes to different conclusions on the change-versus-continuity debate. In contrast to the Scotts, he does consider recent Soviet pronouncements as authoritative as earlier ones were for their day. Also, the book refers liberally to the main corpus of Western works on the topic, noting the numerous differences of interpretation among analysts and associating arguments with specific works.

Although the book is general and often descriptive (and at points reveals poor editing; e.g., *Harry Gelman* becomes *Henry Goldman)*, a basic theme is that Soviet doctrine is different from U.S. doctrine and must be understood on its own terms. At points, the author suggests that doctrine reflects the Soviet Union's political culture, historical experience, belief system, and immediate foreign policy interests. Unfortunately, it is never clear which of these the author regards as most important or whether he thinks they are equally so. Unless one comes to some kind of conclusion about what the doctrine represents, it is impossible to assess the significance of any observed change.

Both of these books contain information useful for students of Soviet military and foreign policy. The Scotts have done an impressive amount of research in primary Soviet sources, from which their readers benefit. Catudal has produced a useful and efficient synopsis of existing scholarship on the issue. Neither work seriously grapples with the difficult issue of exactly what Soviet military doctrine represents and thus the importance of observed changes in it.

WILLIAM C. WOHLFORTH

Princeton University

Fighting to a Finish: The Politics of War Termination in the United States and Japan, 1945. By Leon V. Sigal. Ithaca: Cornell University Press, 1988. 352p. $42.50 cloth, $11.95 paper.

Why did the war in the Pacific continue for at least a year after most responsible officials in Tokyo and Washington recognized the inevitability of U.S. victory? Why, in general, is it so difficult for states to end wars? Leon Sigal believes that this question cannot be answered by treating states as rational unitary actors. Explaining war termination requires an understanding of the interests of different organizations within each capital and of the bureaucratic politics by which organizations try to impress their parochial interests upon the state. Using a wide variety of primary and secondary sources, Sigal details the interests of the powerful actors within Washington and Tokyo (the military services, Foreign Ministries, etc.) and shows how they were not always equivalent to a putative "national interest" of ending the war with an economy of violence, as a rational-decision-making system would. In the final chapter, Sigal explores possible implications for the problem that states might face in terminating a nuclear war short of mutual annihilation.

But is Sigal's account really at odds with a rational-decision-making approach? States may start wars with poorly defined goals and without a clear sense of how the war will end. War aims and the value that decision makers place upon them are clarified through the experience of war. While the balance of capabilities can become obvious early in the game, goals and the resolve to achieve them may expand and contract over time. By this reasoning, the rational criteria for ending a war—when marginal costs exceed marginal benefits, without regard to sunk costs—is probably right by definition. The explanatory "action," of course, lies in understanding how decision makers evaluate marginal costs and benefits for their own side and assess how their counterparts in the enemy state are doing the same.

Even at the end of a long and costly war, Washington and Tokyo were not united in their assessments and evaluations. Internal disagreements arose from many sources: what Ikle *(Every War Must End* [1971]) calls "the fog of military estimates," the peculiarities of individual decision makers, and organizational interests. Sigal has much to say about the latter source. But he may underestimate the power of external events and their strong "structural" constraints to impose themselves on states and supercede parochial interests. For the United States, a central issue in 1945 was imminent Soviet involvement in the war against Japan. As concern about Moscow's behavior in Eastern Europe mounted, it became a priority to minimize the future Soviet role in the Far East. United States decision makers disagreed about precisely how to end the war, and organizations fought to influence that process in a way that would rebound to their advantage; but most shared the belief that the U.S. national interest called for ending the war in a way that would keep the Soviets out of the Far East and certainly out of Japan.

Sigal's description of the organizational politics in Tokyo is fascinating. The atom bomb did not suddenly force divergent perceptions of interests into line. But it did, as Sigal shows so nicely, prompt the emperor to make an unprecedented intervention in cabinet politics, which led directly to surrender. By this reasoning, Sigal's bold rejection of the "common wisdom" that the shock effect of the bomb led to the end of the war (p. 278) is itself subject to question. The atom bomb did not bring the war to a close by fiat, but it did clarify preferences for the emperor, who then broke the political deadlock in Tokyo.

It would be wrong to credit U.S. strategy as having been so clever as to intend this effect. Washington was not well informed about internal politics in Japan. But it is also misleading to *downplay* the degree to which U.S. decision makers expected the bomb to influence Japan's decision to surrender, as Sigal is apt to do. Sigal notes the conclusions of the Strategic Bombing Survey that the bomb itself did not have a decisive impact on Japan's psychological and military resources for continuing the war. But this evidence was not available to Washington until *after* the war. In August 1945 Truman, Stimson, and other top decision makers fully believed that a weapon of unprecedented destructive capability ought to be a source of unprecedented power in world politics. They also believed that they were dealing with an enemy state whose ideological fervor gave it an unusually high tolerance for pain. Given Washington's assessments of Japan's willingness to endure punishment and fight on to improve the terms of surrender, it is not surprising that the United States did not formally offer to retain the throne prior to dropping the bomb. Sigal's own evidence supports the argument that such an offer might have only fortified the "diehards" in Tokyo to fight on for more concessions.

The process of terminating a nuclear war is likely to be no more rational, in the simplistic sense of the term. But there are rational ways to think about the problem. Sigal implicitly adopts the assumption that negotiations to terminate a limited nuclear war would be sensitive to the balance of forces left after blows are exchanged, as in conventional war. Consequently, he worries about the superpowers' ability to assess and to communicate their tolerances for increments of additional pain relative to their war aims as rational unitary actors would; and he fears that these difficulties will make war termination impossible. His prescriptions, that "war termination should be the starting point of any reconsideration of doctrine" (p. 300), that war aims should be specified and made public in advance of any conflict, and that nuclear weapons should be entrusted to a "new low-prestige military organization" (p. 309) seem consistent with his particular theoretical approach; but it is hard to envision how they could be implemented in practice.

There is another, equally "rational" way to think about terminating limited nuclear war. Consider Schelling's argument that nuclear strikes would be primarily a signal of resolve, part of a "competition in risk-taking." War termination would then follow a comparison between the two sides' "critical risks" (Snyder and Diesing, *Conflict among Nations* [1977]) for escalation to mutual assured destruction. Sigal's important work leaves us with the impression that nuclear weapons—or even the credible threat of their use—will clarify states' critical risks with unprecedented force. That is the essence of deterrence. That Sigal's book brings us back to these very important and unanswered questions and offers provocative insights into possible answers is the best testament to the significance and value of his work.

STEVE WEBER

University of California, Berkeley

Alternatives to Anarchy: American Supranationalism since World War II. By Wesley T. Wooley. Bloomington: Indiana University Press, 1988. 256p. $25.00.

Wesley Wooley's analysis of U.S. supranational movements since 1945 tells the important but often overlooked story of U.S. citizens' attempts to create alternatives to a world order dominated by interstate rivalry and conflict. Responding to what were perceived to be the major crises of their day, these citizens sought solutions through international cooperation and the building of supranational institutions. Specifically, *Alternatives to Anarchy* examines the world government crusade of the late 1940s, the Atlantic unionist movement of the 1950s, and a diverse array of activities during the 1970s and early 1980s by individuals spurred to action by concerns about environmental degradation and the desire to build a more just world order. Each of these "movements" was intellectually based in well reasoned arguments about the nature of the crisis in question and the necessity for a supranational response. While led by a core of elites, each enjoyed some degree of more general public support. Yet the popularity of each declined quite quickly and, in the case of the world government and Atlantic unionist movements, totally collapsed.

A major aim of *Alternatives to Anarchy* is to explain why and how these movements arose and why they eventually declined or failed. The analysis reveals that a complex set of factors were at play. Inability to sustain a perception of crisis, public apathy, traditional nationalism, vagueness about the relationship between ends and means, and the inability to demonstrate incremental progress were major obstacles that proved in combination to be too substantial to overcome. Yet Wooley concludes that these supranationalist movements have contributed to how we think about contemporary global relations. They have provided "a warning of future perils, a cogent analysis of the inherent weakness of nation-states, and the planning of a more workable society" (p. 207).

This book is rich in its portrayal of the personalities involved and its description of the historical context of the world government and Atlantic unionist movements. Although much less rich in this regard, the discussion of environmental activists and world order modelers also provides much insight into their motivations and world views. Functionalist orientations toward dealing with global problems provides the rationale for combining the somewhat diverse elements of this third case. The final chapters dealing with these concerns, however, do not possess the same degree of coherence as is found in the discussions of the two supranationalist movements. The activities of the environmental activists and world order proponents do not constitute a movement in the same sense that the others do. A much more probing discussion of the historical evolution of international functionalist activities would have been welcomed in this regard.

Alternatives to Anarchy is well worth reading, especially by younger scholars and other readers who have been misled by a plethora of foreign policy and international relations texts that too often portray supranationalism as being synonymous with idealism. Indeed, one of the strongest elements of the study is Wooley's treatment of the realist foundations of these supranationalist movements. In clearly laying out the assumptions underlying the world federalist and Atlantic unionist movements, he provides the reader with a perspective on realism that has become confused or even lost in many so-called "neo-realist" interpretations of the evolution of postwar global relations and U.S. foreign policy. In this way, Wooley is much in tune with early contemporary realists, like E. H. Carr.

Wooley's analysis is both clearly written and cogently argued. Also, the book is well researched; and the sources are meticulously documented. While some readers may be dismayed by the seemingly forced aggregation of environmental activists and intellectuals searching for a just world order as a movement, this should not detract from the force of Wooley's basic analysis and conclusions. Regardless of personal background or level of expertise in international affairs, the reader may expect to find this book thoroughly engaging.

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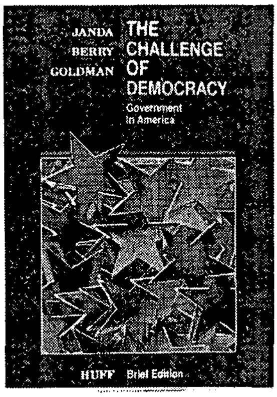

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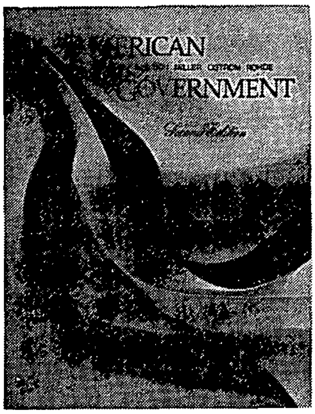

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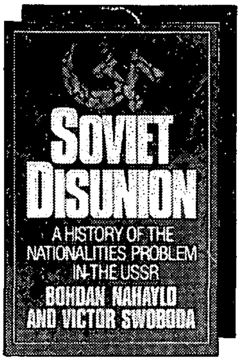
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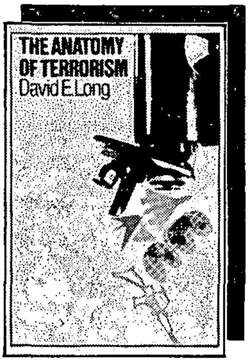

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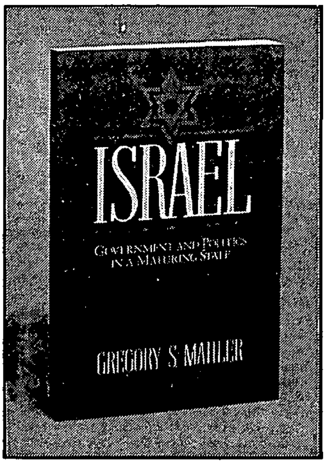
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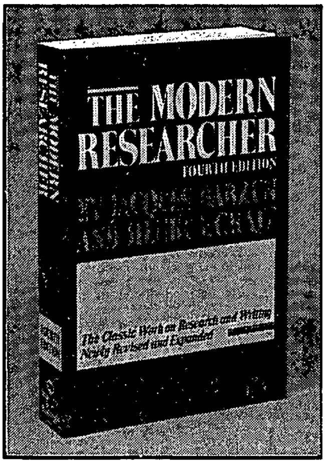
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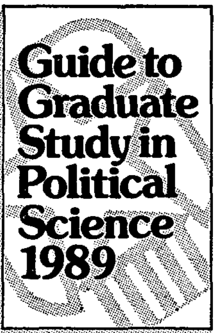

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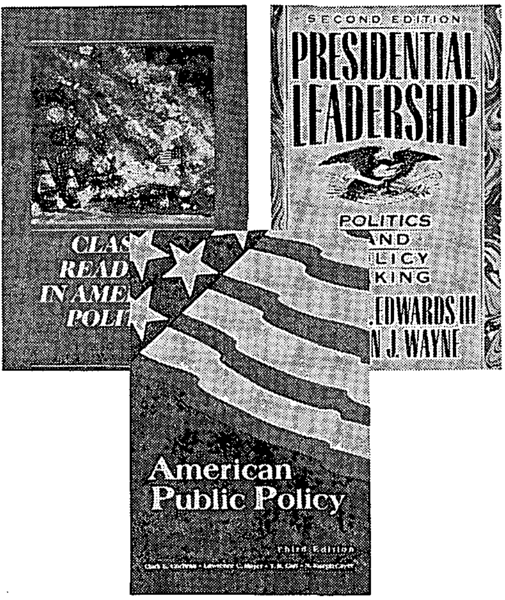

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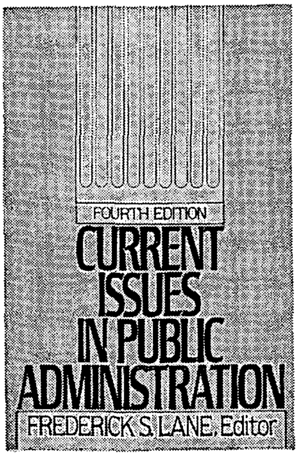

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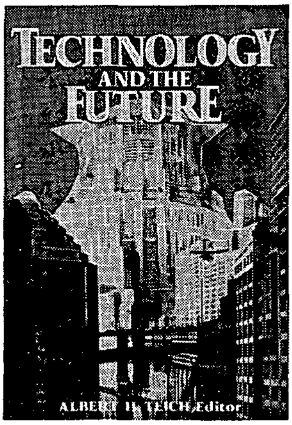

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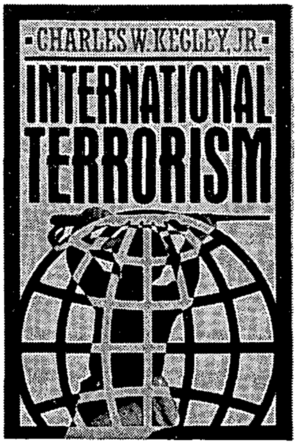

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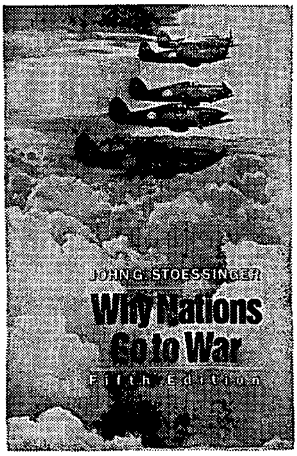

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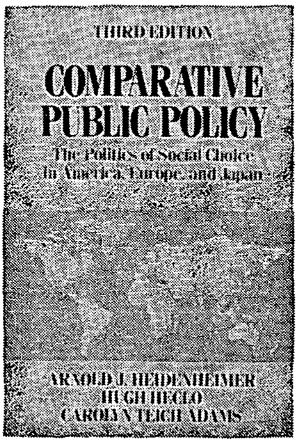
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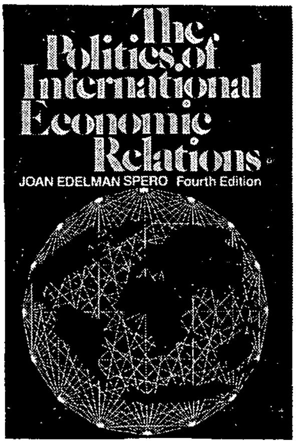
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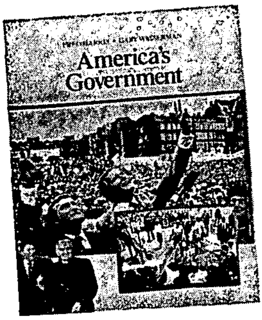
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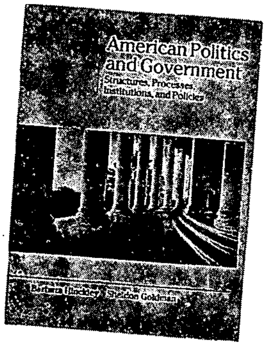
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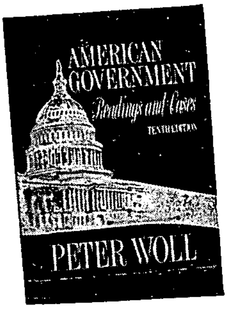
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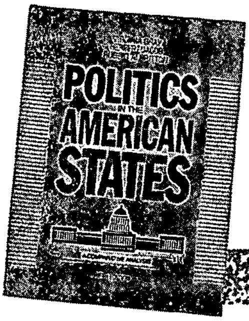
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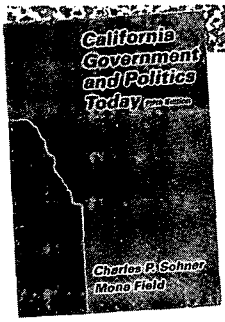
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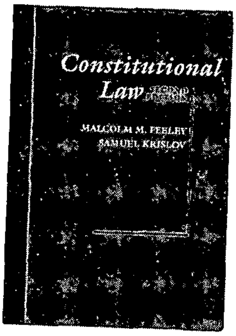
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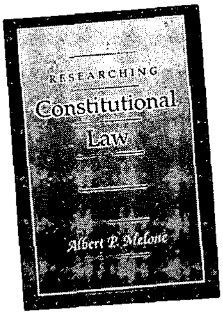
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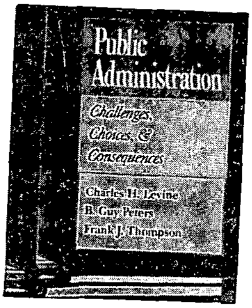
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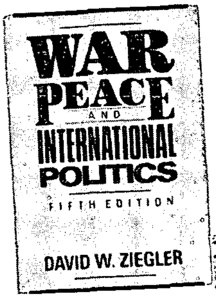
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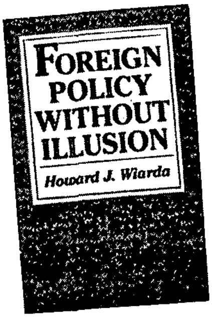
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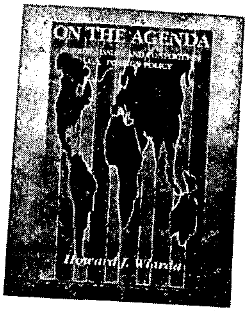
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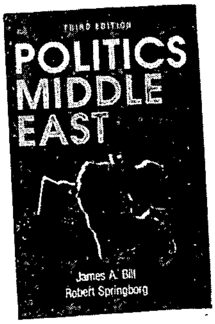
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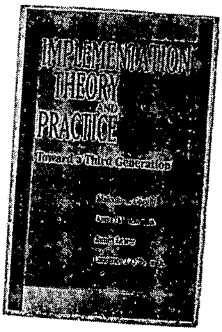
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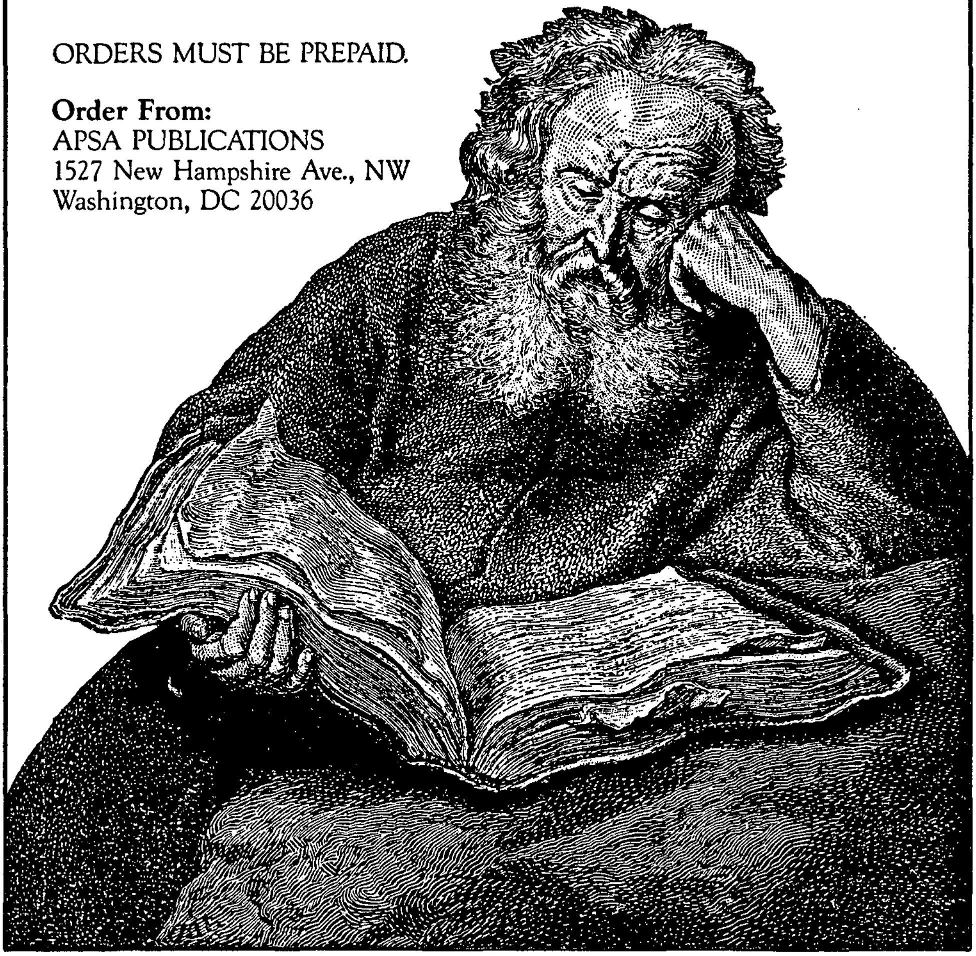

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CONTENTS

ARTICLES

Political Science and the Crisis of Authoritarianism
Lucian W. Pye 3

The International Trade Commission and the Politics of Protectionism
Wendy L. Hansen 21

Opportunity, Willingness, and the Diffusion of War
Randolph M. Siverson & Harvey Starr 47

Legitimacy, Religion, and Nationalism in the Middle East
G. Hossein Razi 69

Communication, Ideology, and Democratic Theory
James F. Bohman 93

Mandates and Policy Outputs: U.S. Party Platforms and Federal Expenditures
Ian Budge & Richard I. Hofferbert 111

Cooperation by Design: Leadership, Structure, and Collective Dilemmas
William T. Bianco & Robert H. Bates 133

Are Congressional Committees Composed of Preference Outliers?
Keith Krehbiel 149

Sequential Voting with Endogenous Voter Forecasts
Dennis Epple & Joseph B. Kadane 165

Missing the Archimedean Point: Liberalism's Institutional Presuppositions
Robert Grafstein 177

Evolutionary Biology and Political Theory
Roger D. Masters 195

RESEARCH NOTES

Victims of Circumstances: Women in Pursuit of Political Office
Timothy Bledsoe & Mary Herring 213

The Electoral Relevance of Local Party Organizations
John P. Frendreis, James L. Gibson & Laura L. Vertz 225

The Strategy of Candidate Ambiguity
Amihai Glazer 237

Asymmetric Information and the Coherence of Legislation: A Correction
David Austen-Smith & William H. Riker 243

CONTROVERSY

Political Culture and Political Change
Herbert H. Werlin & Harry Eckstein 249

BOOK REVIEWS

Science, Rights, and Control of the Body 269
Political Theory 275
American Politics 290
Comparative Politics 330
International Relations 354